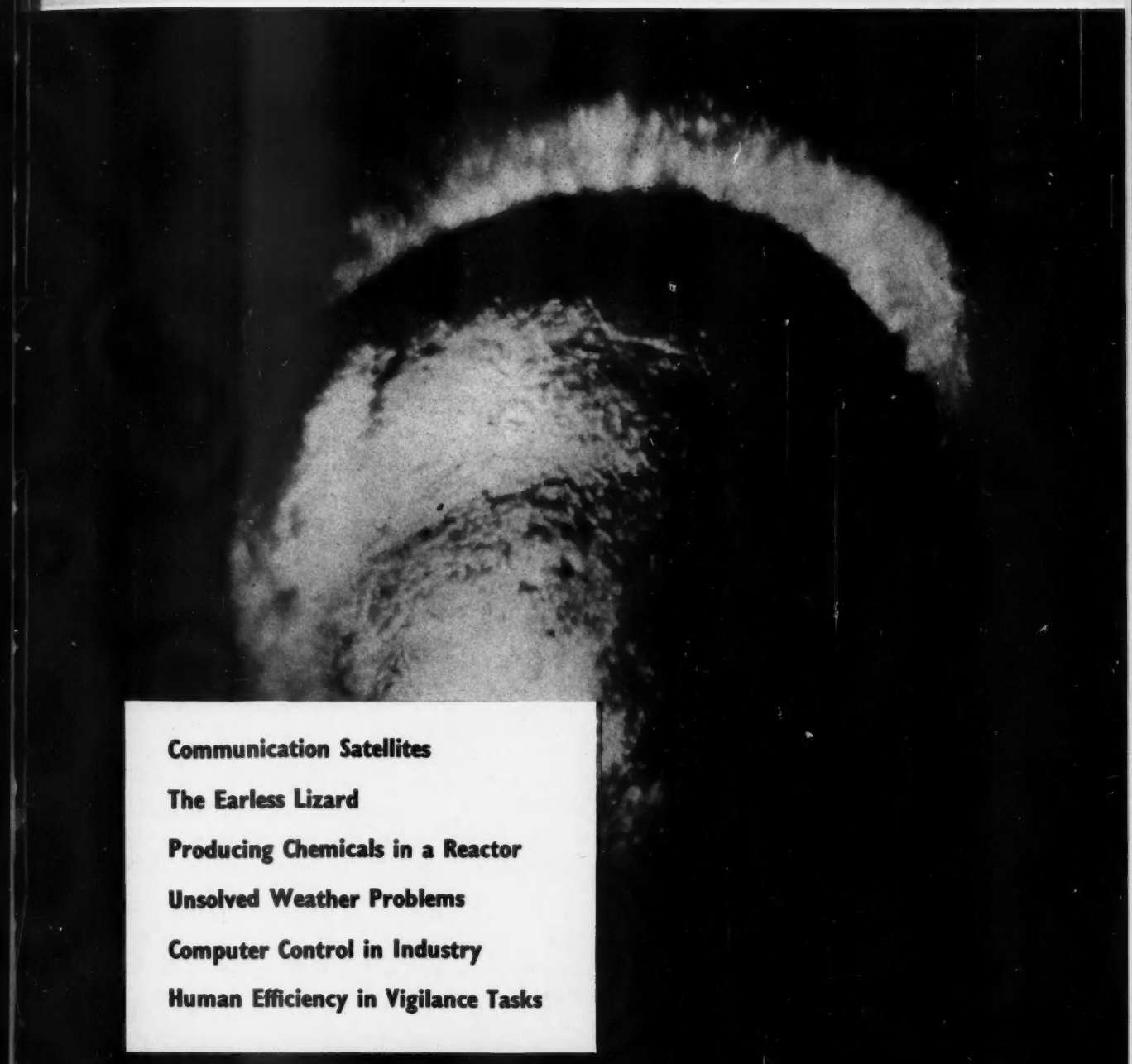
DISCOVERY

THE MAGAZINE OF SCIENTIFIC PROGRESS

JULY 1961 2/6



Communication Satellites

The Earless Lizard

Producing Chemicals in a Reactor

Unsolved Weather Problems

Computer Control in Industry

Human Efficiency in Vigilance Tasks

PRODUCING HUMAN TISSUE IN THE LABORATORY

food flavour

Flavour is one of the more elusive qualities of food: a sensory response to substances in sometimes transitory equilibrium: highly significant: strongly Freudian: so strong that it is difficult to distinguish the purely scientific elements. Yet they must be distinguished if there is to be any understanding of the matter. So one sets off mechanistically, seeking to establish the facts of the matter, and hoping then to postulate how the facts came to be. How far one goes depends on the depth of one's interest—mere curiosity in the flavour, or a desire to make practical use of the facts, or a determination to follow the matter to its problematic end.

The science of flavour substances is part of the science of trace substances, bedevilled in earlier years by the difficulties and doubts of extracting these traces from mountains of raw material, but helped immeasurably in the last decade or so by the development of modern laboratory methods of extraction, separation and identification on a micro-scale. The results have been fascinating, and occasionally frustrating, for many flavour components have been found common to widely different flavours. So the task has been simplified, but not made wholly simple.

In Unilever active interest in food flavour dates back to 1924 when MacLennan started work on the quality of fish. Since 1945 interest has widened and intensified as modern techniques of isolation and identification have been adopted and applied to a variety of flavour problems. Today these techniques are being extended and applied at Port Sunlight by Barrett and Scott to fat flavours; and at Colworth House by Bidmead, Sprott, Welti and others to non-fatty food flavours.

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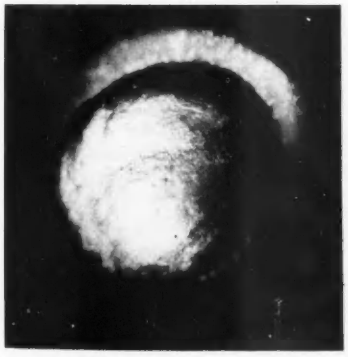
OUR COVER PICTURE



One phase in the "production" of human tissue. See Dr Petrucci's article on page 278

THE PROGRESS OF SCIENCE

RESHUFFLE IN SOVIET SCIENCE LEADERSHIP

The reorganisation of Soviet science and scientific planning, as represented by the setting up of a supreme scientific planning committee with powers superior to and even more all-inclusive than those previously exercised by the Soviet Academy of Sciences (easily the largest and most influential scientific directorate in the world), has been a long time a-coming.

Mr Konstantin Rudnev has now been appointed as Chairman of the new committee with the rank of Deputy Prime Minister. Mr Rudnev has since 1958 been Chairman of the State Committee for Defence Technology.

The decision by the Supreme Soviet to form a state committee to co-ordinate scientific research under a senior administrator, and to strip the Academy of Sciences (run by scientists) of its control over many specialised scientific research institutes, is said to be yet another application of the principle of "decentralisation" which has been an integral part of Mr Kruschev's internal policy. The fact that the present action, first mooted as long ago as June 1959, has taken two years to bring about and then only after an exhaustive public debate among the senior scientists themselves, illustrates the delicacy and respect with which the Soviet leader has felt it necessary to handle this sensitive and articulate group.

The main distinction made between the function of the new co-ordinating committee and that of the redefined Academy of Sciences is to separate the administrative and pure research problems in science; also to establish distinct organisations for the application of science to industry and the pursuit of long-term scientific objectives and basic research.

The immediate purposes of the newly created super-agency as set out in the edict announcing its formation (on April 13) are to strengthen theoretical research that has immediate relevance to the economy, to narrow the gap between research and production, to speed up the assimilation of new scientific advances into the general technology and to improve and supervise the equipping of the specialised institutes.

As a part of this drive to improve the "scientification" of industry and the economy as a whole, the committee is to take over the supervision of a number of the specialised institutes (of which there are 200 in the Soviet Union) and the local branches of the Academy in the outlying districts and autonomous republics of the Union. These will now work direct with the Councils of Ministers and advise on the study, exploitation, and development of natural resources and productive capacity.

The new terms of reference of the Academy are, in contrast, much more specific and scholarly than they have been in the past. It is to concentrate its activity on the conduct of pure science and the formulation of long-range scientific objectives. It will be its task to maintain "scientific and methodological supervision over research" generally. It will also retain its control over the training of scientists.

It now remains to be seen whether a dual system with a Soviet equivalent of the DSIR to co-ordinate applied science and the Soviet version of the Royal Society to supervise pure research proves more efficient than the previous monolithic structure.

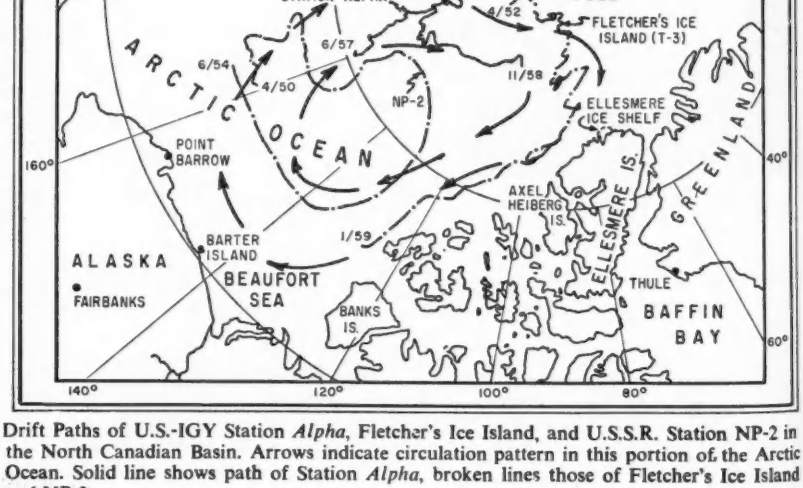
The Academy is having its upheavals too. Nesmeyanov, not surprisingly, has chosen the present time to resign the position as Academy President that he has held for the past ten years. The new man —his name, according to official Soviet sources, was put forward by the retiring president personally—is Prof. Mstislav Keldysh, a mathematician and computer engineer of 50.

The reshuffling of Soviet administrative responsibility for science and the objects which it is intended to fulfill reads like a gloss on the work of two modern novelists preoccupied with the technological revolution, C. P. Snow and the Soviet writer Dudintsev ("Not by Bread Alone"). C. P. Snow, of course, propounds by precept* and, by example, the thesis that administrators are best at administration —even of science—but that scientists should not be too far away since they are the only professional class occupationally obliged to develop and exercise systematically the inhuman capacity for foresight. Dudintsev demonstrates in lavish detail the characteristically Soviet texture of delays, prevarications, intrigue, even sabotage that can face a Soviet inventor of a new chemical process in forcing acceptance of his genuinely superior device on the indifference and inertia of established authorities. No red tape can be quite so red as in a revolutionary bureaucracy, it seems.

GRAVEL ON AN OCEAN BOTTOM

How did sand and gravel, typical of seashores and riverbeds respectively, reach the deep ocean bottom of the Arctic hundreds of miles from the nearest land? This has been the puzzle facing the

* *Most recently in* Science and Government, *OUP, 7s. 6d., April.*



Drift Paths of U.S.-IGY Station *Alpha*, Fletcher's Ice Island, and U.S.S.R. Station NP-2 in the North Canadian Basin. Arrows indicate circulation pattern in this portion of the Arctic Ocean. Solid line shows path of Station *Alpha*, broken lines those of Fletcher's Ice Island and NP-2.

American researchers who have been analysing ocean-bottom samples dredged up in the Central Arctic Basin not far from the North Pole from the IGY drifting station *Alpha*, a temporarily occupied icefloe which circulated in the region between 84° and 85° N, 138° to 152° W during eighteen months of 1957–8.

The high percentage of sands and gravels is the most remarkable feature of the sediments of the Central Arctic Ocean floor, the study of which from Drifting Station *Alpha* is by far the most complete yet carried out in this inaccessible region. They did not get carried there by water: the particles show little or no "rounding" and it has been demonstrated (by P. H. Kuenen in the 1950s for sandstones, Wentworth, 1922, for harder material) that less than half a kilometre of distance travelled through water increases average roundness by several factors.

The distances involved here are many hundreds of kilometres from a suitable sand or gravel source as the crow flies, and, taking into account the circular motion of the Arctic waters, the actual travel path would have been of the order of thousands of kilometres (*see* map).

A fascinating piece of detective work in the laboratories of the Lamont Geological Observatory, Columbia University, over the past two years has established beyond reasonable doubt the provenance, process, and limiting date of this curious episode in geological exchange. The details are published in full in the Air Force Cambridge Research Centre *AFCRC-TN-60-258* and summarised in a recent issue of the U.S. National Academy of Sciences' *IGY Bulletin*. Collating evidence on particle size and roundness, the type and origin of the fine-grain sandstone, limestone and dolomite which predominate, on the striation patterns clearly etched on the harder stuff, on the sea-bed orientation of the pebbles deducible from the manganese deposit on the parts exposed to water, and of the presence in some of the dredge samples of shallow-water fossils, the following picture emerges.

In age, all the sands and gravels dredged appear to be Permo-Carboniferous (that is, formed 205 to 255 million years ago), with the sandstone specimens possibly a little older, that is Permian. There is evidence that all this material originated in shallow-water or off-shore conditions and this is particularly strong for the limestones and dolomites. It appears, therefore, that these Arctic Basin gravels have been ice-rafted to their present location from a coast on which active glaciers reached the shore, and that this happened at some time during the past million years.

It is probable that the material was transported in icebergs that had "calved" directly from glaciers, and was finally dropped in mid-ocean; also that at least part of the material was frozen into the bottom of either grounded icebergs or sea-ice before the "rafts" set off on their journey.

Oceanic circulation in this area (*see* map) and the extent of glaciation in the Pleistocene Age (from about one million years ago to the present) further delimit the possible sources of these gravels.

The circulation pattern in the North Canadian Basin makes it likely that the material was obtained from the northern coast of Greenland, the Canadian Arctic Archipelago, or the northern coast of Alaska. But the northern Alaska and most of the northerly islands of Canada are believed to have been substantially ice-free for the past million years so that source areas are restricted to Axel Heiberg Island, Ellesmere Island, and Greenland's northern coast. This choice of source areas is further endorsed by the fact that one sandstone specimen containing fossil material can be positively identified as Permo-Carboniferous and strata of the same age are found at all three proposed source areas, and, also, that a characteristic white dolomite marble occurs both in the dredge samples and on Axel Heiberg, Ellesmere, and northern Greenland.

In due course, comparison of the bottom gravels with actual samples from Arctic coastal regions may provide definite identification of the source area. In this context the Canadian Polar Continental Shelf Project, under way this summer, will prove valuable. This is the largest and most comprehensive scientific expedition to the Far North the dominion has ever mounted (seventy scientists are taking part at a cost of $1½ million), and one of its objects is to obtain bottom samples from beneath the coastal ice-shelf as well as geological specimens from other coastal areas between Ellesmere and Alaska.

RAISING VIKING WRECKS

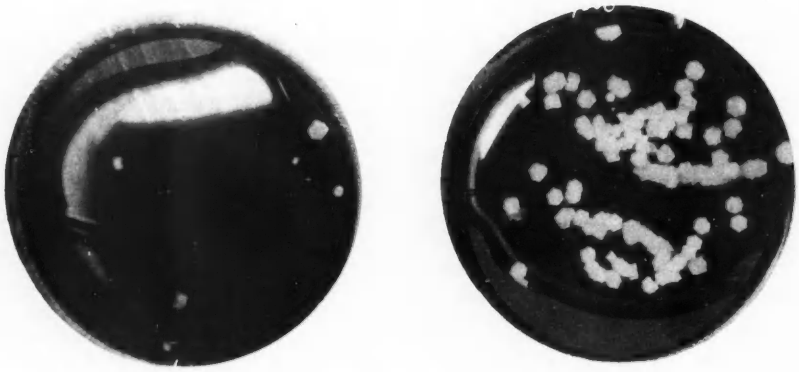
For many years Danish fishermen have reported the presence of a sunken ship filled with stones, at the bottom of the one narrow channel that leads up the Roskilde fiord, at the head of which the ancient city of Roskilde stands. "Queen Margrethe's ship", they have called the wreck; according to local tradition it was sunk there at the command of the thrice-crowned Queen Margrethe of Scandinavia (1387–1412) in order to protect the cathedral town from raiding pirates.

Salvaged fragments, however, suggested to experts that the wreck dated from a considerably earlier period; in fact, that it was a Viking ship. This aroused the interest of the Danish National Museum, and four summers ago some members of the museum staff donned skin-divers' uniform fully expecting to be able to raise the wreck in the fortnight they had free. Their optimism was due to the fact that the blockship lay only 30 in. below the surface. Using fire-hoses, loaned by the local fire-brigade, to clear the mud piled up round the wreck, they soon found their mistake. They were faced not with one wreck but at least six, some of them lying in a row and some sunk on top of each other, and all filled with stones. The whole obstruction measured 150 ft. long and 30 to 50 ft. wide, and was covered with mud, shells, and seaweed.

It has, in fact, taken four summers' diving to complete the underwater survey of the wrecks. The work has been hampered for lack of money—up till now it has been entirely carried out by museum staff with makeshift equipment in the intervals of their other work and with only a short frogman's course behind them. The muddiness of the fiord waters has not helped. At best, underwater visibility has been 5 yds. While working through the ooze that coated the ships' planking the divers had to move blind, by touch only.

This month (July) the work enters a new phase. The entire area of the wrecks is to be enclosed by sheet piling and the water pumped out. Then the task of clearing the remaining stones from the hulls, salvaging these and preserving them for posterity, can go forward faster and more effectively. The oak of which the thousand-year-old ships are made has proved to be extremely well preserved, but the wooden rivets which hold the ships together can no longer be trusted, and a danger during the underwater phase was that small parts would be carried away by the current when they were freed from the weight of stones.

The wrecks in the Roskilde fiord are unique in several ways. All the sunken ships so far studied are merchant ships, not war vessels like almost all the Viking ships of which substantial remains have been found elsewhere (for instance, in the ship-burials in Norway). Little or nothing is known of the trading activities of the Vikings, much better known for their piratical and warlike exploits. The construction of these ships may give a clue to goods they carried and the people with whom the Vikings did business. Their customers must have lain at some



Freezing nuclei detected at 60,000 feet during U-2 flights in Australia.

A pair of filters was carried on each flight, one being exposed to the air stream, and the other not. At the end of each flight, the filters were "developed" at a temperature of −15°C. Ice crystals grew on the nuclei collected on the exposed filters (right), none or very few on the filters which had not been exposed (left).

considerable distance, for all but one of the ships are sailing vessels. It is expected that one at least of the Roskilde ships can be reconstructed in complete detail; this will fill a gap in scholars' knowledge of how the art of shipbuilding developed in the Viking period.

There is one mystery that even the complete salvage of all the Roskilde wrecks may never solve: who was the enemy that these Norse sea-wolves feared so much that they were prepared to sink a merchant fleet to block the invaders' passage up the fiord to their settlement?

DO METEORS CAUSE RAIN?

What causes rain? After 10 million years' experience of it we still do not really know. The incompetence of man's efforts to produce it when there is a drought, or stop it when inundation threatens, underlines this. Mr Macmillan won the reputation of rainmaker in parts of Africa when he was there. The greatest precipitation ever described, that which floated Noah's ark in the book of Genesis, was caused—according to the same source—by the wickedness of man.

Some years ago, Dr E. G. Brown, the unconventional physicist who is head of the Radio Physics Division of Australia's Commonwealth Scientific and Industrial Research Organisation (CSIRO), astonished the meteorological profession by the proposal that there was an intimate connexion between periods of heavy rainfall and meteor showers.

This was an intriguing hypothesis and extremely hard either to prove or disprove. It was a temptation to wish it true since it would greatly simplify artificial rain-making operations if successful "seeding" could be accomplished with metallic or glassy granules (such as meteorites are made of) in lieu of the expensive chemicals that have so far proved the most promising in seeding operations. (*See* the August 1960 issue of DISCOVERY for an article on "Controlling and Forecasting Weather".)

One way to settle the matter is through a statistical analysis of the correlation between regularly occurring rainfall peaks from all over the world with the incidence of meteor showers. This has been going on, and confirmation of Bowen's hypothesis has recently come from the Russians; but this work takes a long time, since rainfall records have not been found available from a number of important areas and because there have been many additional meteor showers added to the original total in the last few years.

The visit to Australia in recent months of high-flying U-2 jet aircraft for the purpose of monitoring high-level radioactive fall-out in the southern hemisphere has just offered a unique opportunity for testing the theory in quite another way.

It is well known that one prerequisite of rain is some kind of nuclei around which the ice forms, eventually leading to the formation of the droplets that fall to earth in the way we are all familiar. What kind of material provides the core for this crystallisation far up in the atmosphere and how far up does it occur? Bowen's hypothesis required the bulk of these nucleating particles to be meteoritic in origin and therefore to be coming from outside the atmosphere.

A way of checking up on the origin of these freezing nuclei is to measure their number at greater and greater heights above the earth. If the particles originate as dust particles drawn up from the earth's surface, their number will drop off sharply at the tropopause and very few indeed will be found above 30,000 to 35,000 ft. If the particles are of extra-terrestrial origin, there will be no significant change above or below the tropopause.

The great "ceiling" of the U-2 aircraft has made it possible to sample the freezing nuclei up to heights of 60,000 ft. directly. The Americans have with ready generosity flown special filters on a number of their flights from Australia. It has been found there are considerable numbers of highly active nuclei up to heights of 60,000 ft. and beyond, and this supports the view that they have come in from outside the earth's atmosphere.

The next stage is to push the sampling up to 100,000 ft.; this will probably be done by means of balloons. Also to try to obtain chemical identification of the nucleating particles, to see if they are terrestrial dust or extra-terrestrial metal or silicate.

RE-CALCULATING *pi*

Mr N. D. Robinson, a mathematician working at the EMI electronics laboratory at Hayes, Middlesex, has programmed an Emidec 1100 computer to calculate the precise value of π, otherwise the ratio of a circle's diameter to its circumference, the common approximation of which is 3·14. He pursued the calculation to 10,880 decimal places but still found a remainder. Mathematicians have shown theoretically that π cannot be worked out exactly.

It is estimated that the 35 million separate calculations involved in carrying the value to 10,880 decimal places, if done "by hand", would have occupied one person, working a 40-hour week, for 65 years—with a fortnight's holiday a year. The Emidec 1100 performed the task in thirteen hours of computing time. The experiment was undertaken as a machine reliability test. It is thought that π has never before been worked out to this degree of accuracy.

In 1853, π was calculated to 530 decimal places by a Mr W. Shanks. Twenty years later he continued his calculations as far as 707 places, but it has now been shown that he made a mistake at the 530th place.

SPACE RESEARCH

ANGELA CROOME

EUROPEAN SCIENTISTS DETERMINED

There is now no doubt at all that European nations will, within a limited number of years (say five), have their own artificial satellites in orbit. This despite the astonishingly hesitant course taken by the *Blue Streak* satellite project backed by British and French official aviation interests. It is no longer the vested interests but the scientists of Western Europe that are setting the pace. They know their own minds and can therefore act with authority and vigour.

The European Space Research Organisation (ESRO), the body these men formed last summer, is now not only official, it has a year's budget in the coffers and it has a substantially agreed draft research programme, part of which could go into operation within nine months and probably will.

All this became finally clear at the last meeting of ESRO, which took place at Scheveningen, Holland, a month ago. Here the ESRO technical committee (which has been given precisely two months to get its work done) presented a specific space research programme appropriate to the twelve member-countries, a time-scale for this, and made recommendations for the budget and organisation required to implement this programme.

When the detailed assessment of the situation is complete (probably about October), an inter-governmental conference will be called to set up the Organisation as a permanency, embodied perhaps in a treaty. The cost of the permanent organisation is estimated at about £6 million a year, the amount to be divided between member-countries in proportion to their national incomes.

Because it is impossible to shape a legal, administrative, or financial policy without knowing what you are financing or legislating for, the technical group was charged with preparing their report in two parts; one part to deal with "a programme of selected projects to be undertaken as soon as possible (that is within a year from now)"; the second part was concerned with long-term projects, including satellite programmes.

Among the long-term projects considered feasible and desirable was the sending of a satellite round the Moon, and the operation of an astronomical satellite to observe the stars from above the atmosphere—which constitutes the main impediment to further astronomical discoveries on Earth. Spokesmen made it clear that the programme did not envisage launching "just one satellite"; "there was no end to it".

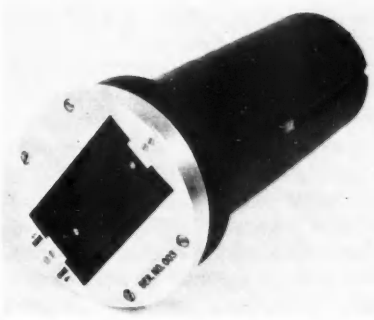
Expanding on the kind of thing which European nations could co-operate on immediately, Prof. Sir Harrie Massey, the ESRO president, cited satellite-tracking in Europe, which is "extremely ill-organised at present". Another field that could be tackled immediately is the expansion of the vertical rocket programme using already developed rockets such as the British *Skylark* and the French *Véronique*.

The programme proposes that there should be a rocket range established within the boundaries of Europe so that the sampling of the upper atmosphere can be extended to additional latitudes. At present the British use Woomera in the southern hemisphere sub-tropics, and the French the Sahara.

Possibly the range the Swedes are developing near the Arctic Circle could be made to accommodate ESRO rocket work if enough time can be spared from quasi-military tests. This is a scientifically more interesting area of the upper atmosphere than that above the Italians' Sardinian range.

The type of booster rocket to be used for getting Europe's satellites into orbit remains an open question. If—or should one say, when?—the *Blue Streak* plan goes through, the European space group would be predisposed to make use of it, their object being to "be as self-sufficient as possible". But they do not intend to let their hands be tied by this problem. If *Blue Streak* is not available, then they will go elsewhere—most likely buy American boosters "off the shelf" in line with Mr Dean Rusk's recent tempting offer.

OZONE SCANNER



The Meteorological Office's ozone scanning unit shortly to be tested in a *Skylark* rocket before the final version is prepared for flying in the *UKII* satellite. *UKII* is the official name of the second American-launched *Scout* satellite to carry British instruments into orbit, and is tentatively scheduled for next year. The experiments for *UKII* were officially announced recently, at the same time as the final instruments for *UKI* were dispatched to the U.S. The ozone scanner is the most unusual experiment of the three approved for *NKII*. It will yield data on the mixing of the upper atmosphere by a novel technique.

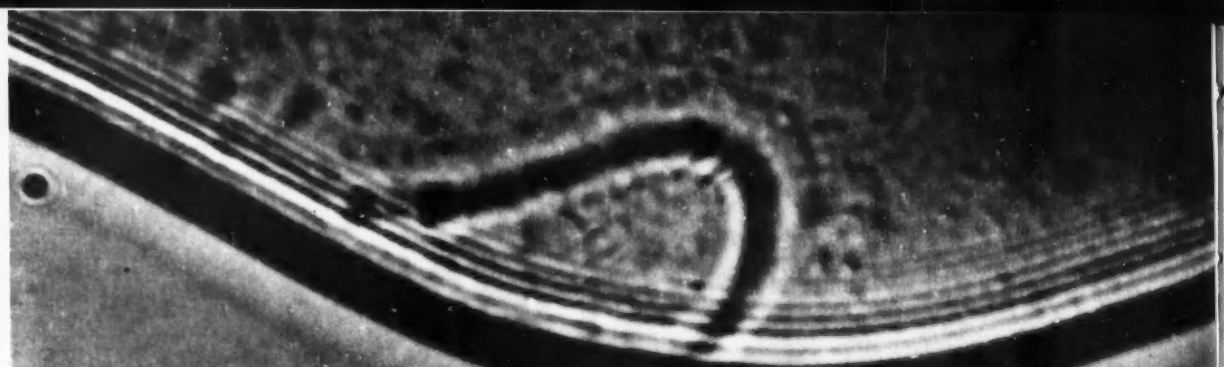
LIGHTING A LAMP IN SPACE

An interesting rocket experiment is expected from the lively French upper-atmosphere research group, led by Prof. Jean Blamont of the Sorbonne and Meudon Observatory, this summer. The experiment is being launched above the Sahara range (Colomb-Bechar) by the medium-range French rocket *Véronique*, and may be expected at any time now.

At a height above 100 miles a charge of 100 lb. of TNT is exploded in the extremely tenuous upper atmosphere and a ground network of highly sensitive magnetometers will monitor the explosion for correlated changes in the local magnetic field. It is far from clear why shock-waves in the near-vacuum of space should affect the Earth's magnetic field; one object of the experiment is to make an assessment of this mechanism if it is found to exist, in conjunction with such a comparatively small explosive charge.

Magnetic stations in the Pacific at the time of the last series of American nuclear tests (several of which took place at great altitude) observed considerable reactions in the magnetic field at ground-level; but the explosive charges involved there were measured in megaton equivalents of TNT, not in kilograms, as in this case. Visual observations of the colour and brightness of the explosion in the night sky as well as extrapolation from the magnetic effects will yield information on the density and chemical composition of the upper atmosphere at these heights, about which extremely little is still known.

Blamont made a preliminary test of this experiment from the Sahara last year, and although the magnetic data was inconclusive (due probably to insufficiently sensitive and numerous magnetometers on the ground) the effect was at least spectacular. The glow produced in the sky was many times brighter and larger than a full Moon, lasted a considerable time, and faded in a corruscation of vivid greens.



See Fig. 19.

These photographs, many of them never before published, show the successive stages in an experiment involving fertilisation and development of the human embryo outside the body. The object was to produce transplantable human tissue.

PRODUCING TRANSPLANTABLE HUMAN TISSUE IN THE LABORATORY

DANIELE PETRUCCI

From the experiments on rabbits and guinea-pigs of Shenk in 1880, and Onanoff in 1892, to the current researches of Chang and Thibault with rabbits, some thirteen investigators have studied the fertilisation and development of the embryo *in vitro*. Only three—Rock, Shettles, and Petrov—have, however, conducted experiments on human cells.

Rock obtained the first division of the egg nucleus. He succeeded on three occasions out of 138 attempts, but obtained no further development of the cell. Shettles published in 1960 a monograph summarising all the papers he wrote between 1953 and 1958. In this book he shows, with eight microphotographs, human egg fecundation with transformation of the zygote from the first division to complete segmentation at the morula stage.

AVAILABILITY OF SEX GAMETES

Both in animal and in man, eggs can be obtained either from the ovaries or the uterine tubes. Spermatozoa are obtainable from the epididymis, the *vas deferens*, and as a result of ejaculation. They may be matured or not. Various fluids have been used by different authors for nourishing the fertilised egg. They include genital mucus, Ringer's solution, both homologous and heterologous serum, blood plasma without embryonic extract, Tyrode solution, Ringer-Locke solution of *p*H 7·3–7·5, and follicular fluid. Very few investigators have given attention to the matter of temperature regulation and no one to that of respiratory exchanges.

THE PURPOSE AND POTENTIALITIES OF OUR RESEARCH

The specific purpose of our research was initially to build tissues which, in contrast to adult tissue, would be transplantable and acceptable by an adult organism. In early life, embryonic tissues have weak antigenic properties. Small fragments of embryonic tissue represent entire future organs and do not need a true arteri-venous circulation for their own feeding, but if properly transplanted they can develop a specific vascular network.

When an organ changes its host, it loses not only its lymphatic, arterial, and venous connexions but nervous and mechanical ones as well. For example, the kidney, to survive and function, needs not only an input and

The experiment was conducted in Dr Petrucci's Bologna Laboratory early this year. The text of this article was translated, in an interview, from Dr Petrucci's original Italian, and checked for vocabulary by an English surgeon and embryologist.

output of blood but neurovascular connexions for regulating the "filtration" of the blood, and also a structure for collecting the filtration products excreted.

Certain glandular organs have simpler connexions with the host, and the feeding vessels that take them nutriment also carry away their hormones to the rest of the body. This is especially true for single-function glands.

The glands that have a double function, such as the ovaries and testicles which simultaneously produce hormones and gametes, must have a specific topography to be able to convey eggs and spermatozoa to the proper site of utilisation. If these glands are transplanted, they can carry on only their secretive function.

The hypophysis, although fulfilling several hormonal functions, seems to be the most easily transplantable of all the glands. The results obtainable from transplantation of the hypophysis of a calf to man, as a temporary supply of hormones, are well known.

From this it is easily understandable how the chances of a glandular transplant's surviving will be improved if we obtain the gland from an embryo that is first cultivated outside the maternal body in plasma from pooled pregnant women's blood, second exsanguinated, and third irrigated with homologous plasma belonging to the new host. For these reasons we started our researches at the stages of fecundation *in vitro* and cultivation of the zygotes until the organs in which we were interested became differentiated.

At the beginning of our researches, we paid a great deal of attention both to the existing body of knowledge and also to the ethical and theoretical issues. The purpose of our research has been already clearly defined. Moreover, it has been and is important that those individuals from whom gametes have been obtained know that these are not being improperly used.

TECHNIQUE

The conditions for duplicating the proper environment are both physical and chemical: such factors as temperature, pressure, viscosity, hormonal and protein levels must be controlled. Reproductive cells of the two sexes must be prepared for meeting, and the union should take place under conditions as close as possible to the natural ones. These must be under close control if the zygote is to survive for the maximum period of time.

THE EQUIPMENT

Fig. 1 shows the assembly of the "biological cradle". Plasma collected from women who are more or less at the same stage of pregnancy as our experiment, is dripped into the Perspex container from the top. The dripping device is made to give a perfect imitation of the arterial capillary dynamic pressure, slight pulsations being produced by the intermittent introduction of liquid.

The dynamic pressure is accurately measured with the condenser-type microphone shown in Fig. 2; the static pressure is measured with a conventional pressure gauge.

The components of the "cradle" are shown in Fig. 3. The egg is placed between the two chamois discs shown at *A*, which duplicate the site of nidation in the endometrium. The discs are then placed on top of a tantalum wire net which is then placed on one of the rings shown at *B*, the size of ring depending on the species of embryo under observation. The wire net and ring are only to support the chamois discs but the size of the hole in the ring also determines the area through which the plasma flows through the discs.

With this arrangement, the developing zygote and embryo are photographed by lifting the top chamois disc, but other "cradles" have been used in which the developing zygote and embryo could be photographed continuously through the transparent walls as shown in Figs. 4 and 5. The "cradle" shown in Fig. 4 (15 mm. by 3 mm.) was only large enough to keep the zygote (the small white spot in the centre) alive for two weeks. The cradle shown in Fig. 5 was considerably larger (45 mm. in diameter) and could keep the embryo alive for eight weeks.

An even smaller cradle made out of a microscope slide with a 1 mm. hole has been built for two-day experiments.

FIG. 1

FIG. 2

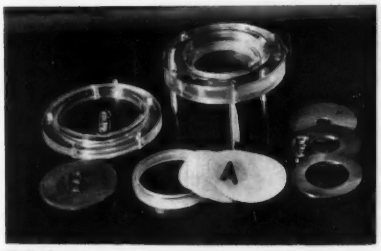


FIG. 3

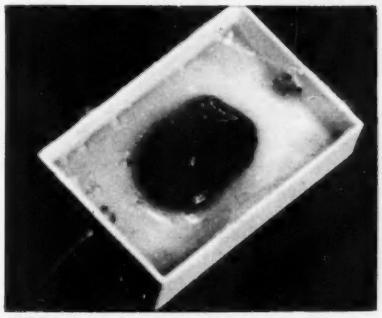


FIG. 4

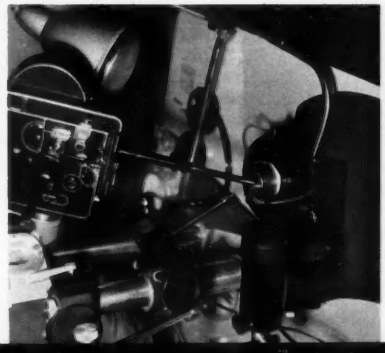


FIG. 5

REMOVAL OF THE OVUM

Fig. 6 shows a matured follicle that had been removed from a surgical specimen obtained at total hysterectomy for cancer. This follicle, about 10 mm. in diameter, was about to burst, ejecting the flake (arrow) containing the ovum into the uterine tube.

Fig. 7 is a close-up of the flake before the bursting of the follicle and Fig. 8 is a view of the flake after ejection from the follicle showing the ovum inside (about 0·15 mm. in diameter). Shown in Fig. 9 is the large nucleus of the naked egg with the two polar bodies below.

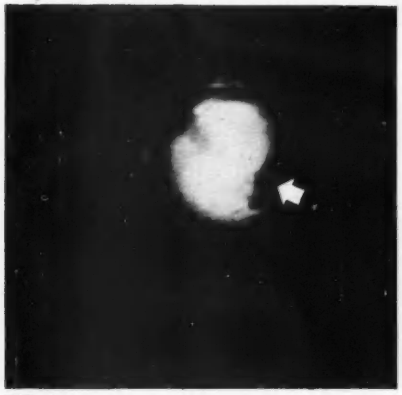


FIG. 6

FIG. 7

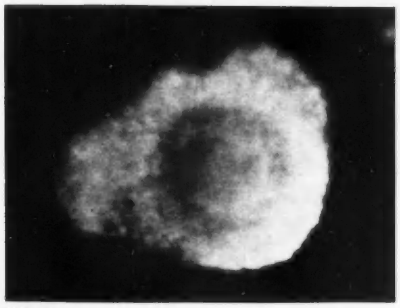


FIG. 8

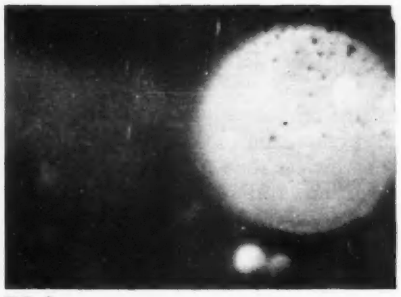


FIG. 9

INTRODUCTION OF THE SPERMATOZOA

After injecting the egg into the "cradle" and washing with a saline solution and hyaluronidase (a diffusing enzyme), the spermatozoa are introduced, as shown in Fig. 10. The egg is seen at the right.

Figs. 11 and 12 are close-ups of the spermatozoa. In Fig. 12, the oval-headed spermatozoon at the left is the x-carrying (female-producing) type and the one at the right is the y-carrying (male-producing) type. The heads are about three to four microns across.

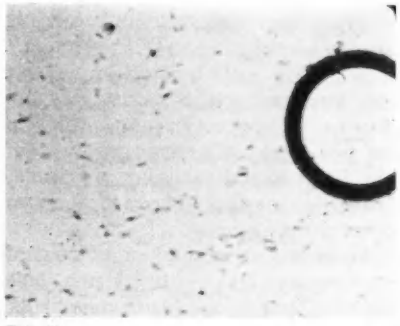


FIG. 10

FIG. 11

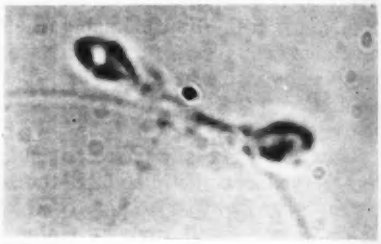


FIG. 12

FERTILISATION

Figs. 13 to 16 are a sequence of photographs, spaced at approximately 40-second intervals, showing the penetration of the head of the spermatozoon through the vitelline membrane. Fig. 17 is a close-up showing the piercing of the vitelline membrane by the spermatozoon.

Fig. 18 shows the head of the spermatozoon inside the cytoplasm, after penetration, directed towards the female pronucleus. The tail of the penetrating spermatozoon sometimes gets part way through the membrane but is soon abandoned, as shown in Fig. 19. On its journey towards the pronucleus, the head becomes enlarged and divides as shown in Figs. 20 to 22, which were taken four or five minutes apart.

In Fig. 23, the head is about to fuse with the pronucleus of the egg. Fig. 24 shows the fusion of the two. Fig. 25 shows the expulsion of the polar body soon after the fusion or fecundation.

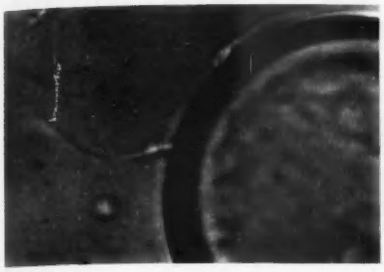


FIG. 13

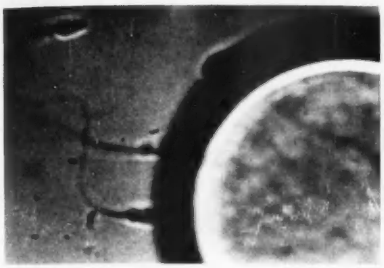


FIG. 14

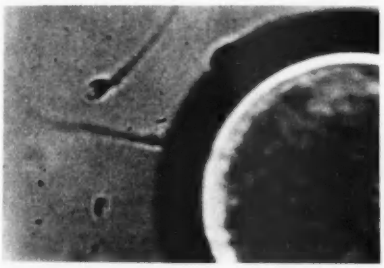


FIG. 15

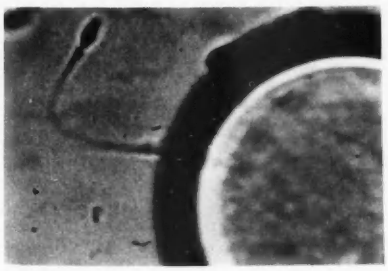


FIG. 16

FIG. 17

FIG. 18

FIG. 19

FIG. 20

FIG. 21

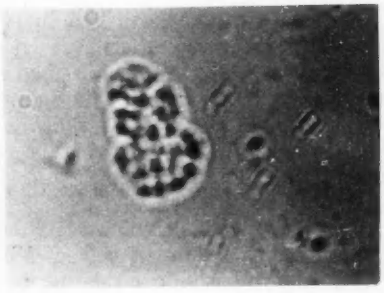


FIG. 22

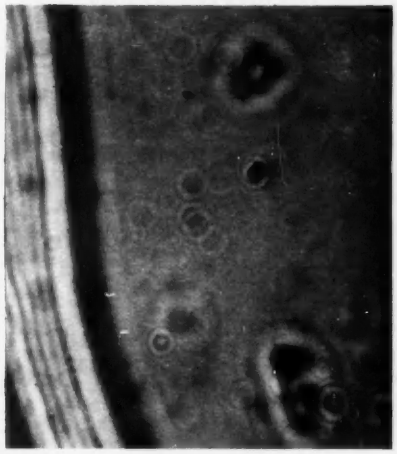


FIG. 23

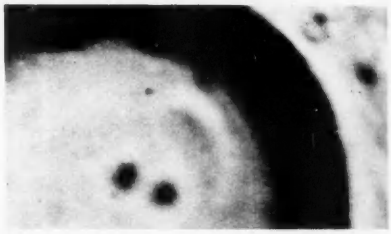


FIG. 24

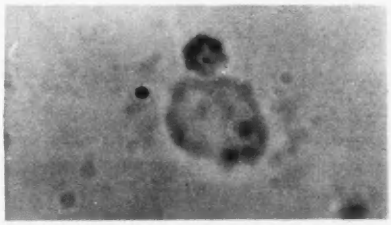


FIG. 25

CELL DIVISION

Fig. 26 shows the beginning of cell division. In Fig. 27, the first segmentation is completed. In Fig. 28, the first segmentation is shown in close-up with the phase-contrast microscope. The 46

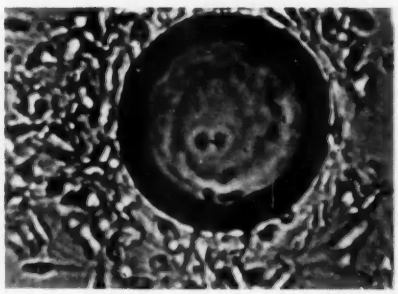


FIG. 26

chromosomes can be seen in each segment. On the right-hand side, we can see the expelled polar body. The two black spots at the line of segmentation are the x-chromosomes that seem to be the residue of the two former pronuclei (the head of the spermatozoon and the pronucleus of the egg) which can be considered the mark of the female chromosomal sex.

Further division is shown in Fig. 29. Division then continues and Fig. 30 shows the advanced morular stage (of one cell) where the vitelline membrane is broken (at the end of seven days).

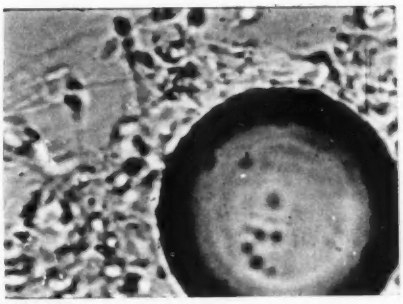


FIG. 29

FIG. 30

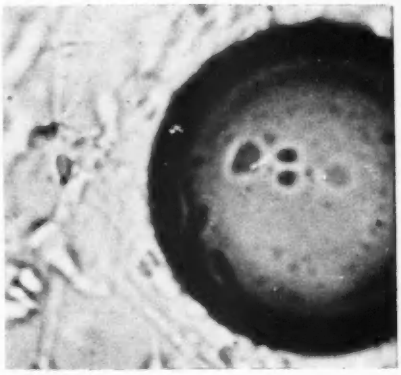


FIG. 27

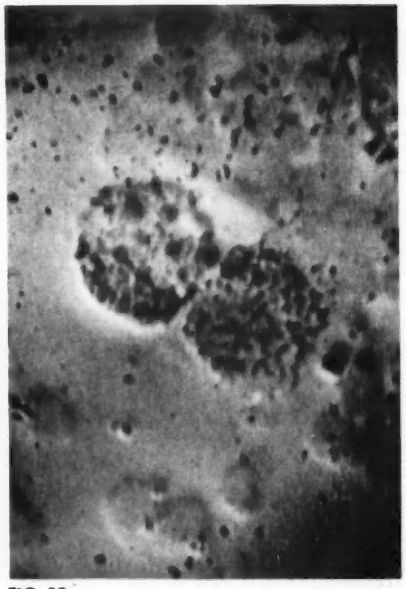


FIG. 28

GROWTH OF RECOGNISABLE FEATURES

Fig. 31 shows how the mass of dividing cells becomes eccentric and reaches the stage of blastula. The blastula is composed of the embryoblast shown in Fig. 32 and the trophoblast shown in Fig. 33. At this point, the blastocyst pushes out cells which become placentar villi as can be seen in Fig. 34.

At the end of the second week, the embryonal disc is formed, as shown in Fig. 36. Inside the embryonal disc, at the 20th day, we have already formed the neural tube, the midgut and the septum transversum, as seen in Fig. 37. At the beginning of the fourth week, somite formation starts. Seven pairs of somites can be seen in Fig. 38. In Fig. 39, at the late somite stage, we can see established the stomodaum, aortic arches, dorsal aorta, the pericardial cavity and the septum transversum.

Using deep immersion in plasma, with single frame technique, we have then recorded the next twenty-nine days of development (29th to 58th day), as shown in the sequence from Figs. 40 to 47.

FIG. 31

FIG. 32

FIG. 33

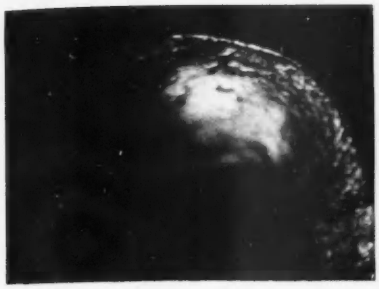


FIG. 34

FIG. 35

FIG. 36

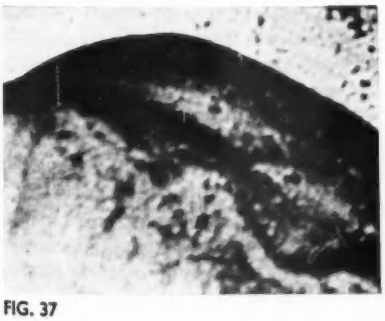


FIG. 37

FIG. 38

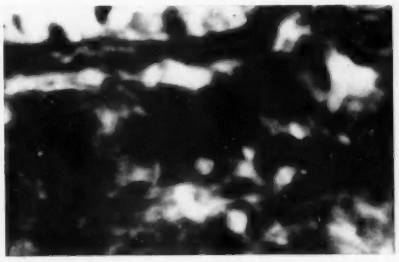


FIG. 39

FIG. 40

FIG. 41

FIG. 42

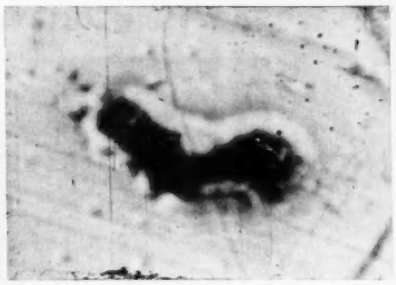


FIG. 43

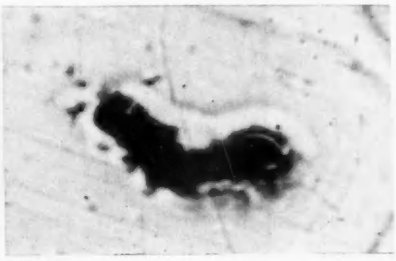


FIG. 44

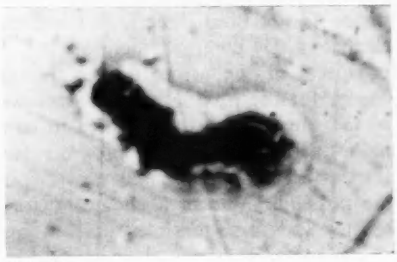


FIG. 45

FIG. 46

FIG. 47

PRODUCING CHEMICALS WITH NUCLEAR FISSION ENERGY

G. R. HALL

The production of chemicals using nuclear fission energy appears possible. But prospects are that both power and chemicals will be produced at the same time—when the many technical problems now being studied are solved.

In a nuclear power station the reactor produces heat and then the heat produces steam which drives turbines. In other words, the energy released in fission is not used directly to produce electricity. By contrast, there are some prospects of the direct use of fission energy for the production of chemicals in the nuclear reactor. Studies in this field have only been in progress for some five years, but we may now assess the possible value of a chemo-nuclear reactor and some feasible applications.

The term "chemo-nuclear reactor" is used here to refer to the direct use of fission energy in promoting chemical reactions and not solely to the use of gamma radiation within the reactor.

If it were possible to utilise the whole of the energy obtained from the fissioning of one gram of uranium-235, we would obtain 21,900 kWh of energy. Even in the more advanced thermal reactor power stations at present under construction, it is unlikely that thermal efficiencies exceeding 30% will be attained, and so a large proportion of the fission energy is wasted. There is, of course, a similar wastage in the burning of fossil fuels in conventional power stations.

Many other methods of utilising some of the fission energy have been suggested. One is to use the fission products, after separation from the irradiated uranium fuel, as sources of beta and gamma radiation. Even if this could be completely utilised, only some 5% of the fission energy would be available. Many of the fission products, however, are short-lived and it is also difficult to use the gamma emitters efficiently, because gamma radiation is so penetrating and not readily absorbed in chemical systems. It is therefore only possible to make use of a very small fraction of the fission energy in this way. (*The box on p. 288 describes the distribution of energy in nuclear fission*).

A more attractive possibility is to attempt direct absorption of the kinetic energy of fission fragments in a chemical system. About 84% of the total fission energy might then be available. The two fission fragments are ejected by the uranium atom with a mean total kinetic energy of about 126 MeV and each fragment is highly charged—up to at least 20 units of charge immediately after production. If the uranium atom is surrounded by air at atmospheric pressure, the fission fragments can travel about 2 cm. and along their path they lose their energy and produce intense ionisation. In a liquid such as water, they can travel only 0·0025 cm., whereas in a solid such as uranium dioxide (UO_2) they lose their energy in a distance of 0·001 cm.

The fission fragments may be considered as another source of radiation energy. Because of the short distances which the fragments can travel in various media, the rate of release of energy is very high. But the short track presents many difficulties, particularly as to the ways in which the fissile material can be represented to the chemical system.

POSSIBLE REACTOR FUELS

The only method by which the theoretical fission fragment energy could be released to gaseous reactants would be by using uranium hexafluoride (UF_6) in which the uranium was highly enriched in the fissile uranium-235 isotope.

Such a fuel would lead to a variety of unwanted side reactions with fluorine. There is also no experience, except possibly in Russia, of gas-fuelled reactors. These would undoubtedly lead to many difficult operational problems.

Conventional solid reactor fuels would be quite useless because, as we

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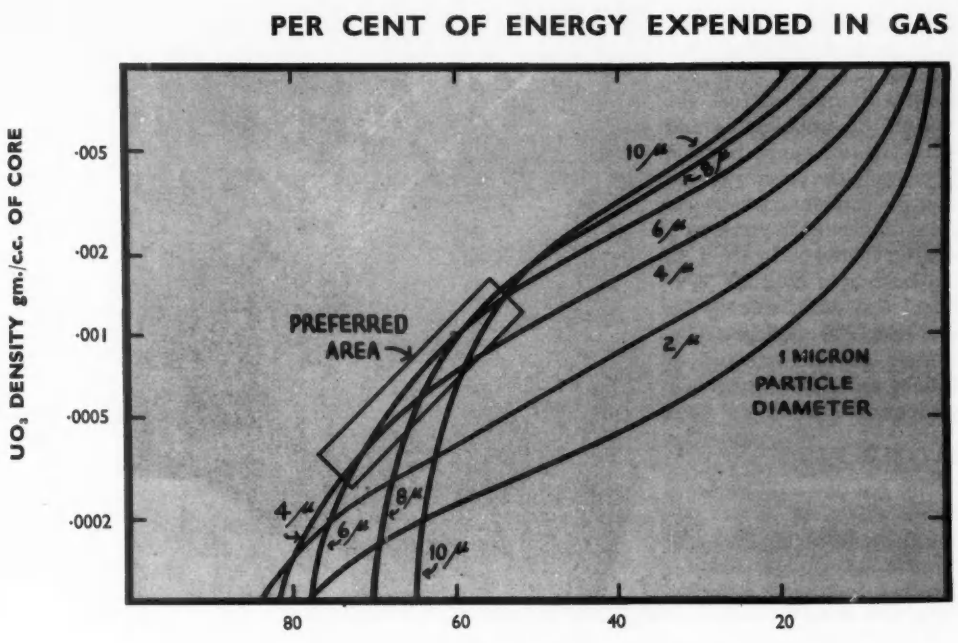

FIG. 1. Optimum fuel particle size for reactor that uses fission fragment recoil energy to synthesise reactant gases at 20 atmospheres pressure and at 600°C. Optimum fuel particle size is 4–10 μ and 50–70% fission fragment energy is deposited in the gas. (Reproduced from *Nucleonics*, Feb. 1961, p. 49.)

have seen, the fission fragments have such a short range in solids that only those fragments produced on or very close to the surface of a solid fuel might reach the chemical reactants. These would, however, all be stopped by the can which contained the fuel.

The average fission fragment has a range of only 10 μ in UO_2. If the fuel is to be a solid, then it is essential for one dimension to be very small and preferably 2 μ or less. There are several methods of fuel arrangement which provide this. One is to support a thin film of uranium oxide on a backing plate and allow the chemical reactant gases to flow past. The distance of separation between plates will depend on the pressure of the gas system—at atmospheric pressure a separation of 2 cm. would be essential to prevent partial re-entry of fission fragments into adjacent plates. On the other hand, by increasing gas pressures, the fission fragment track is reduced; for example, to about 0·2 cm. at 10 atmospheres in

The construction of a reactor with fuel coated on plates presents many difficulties. Dawson[1] has calculated

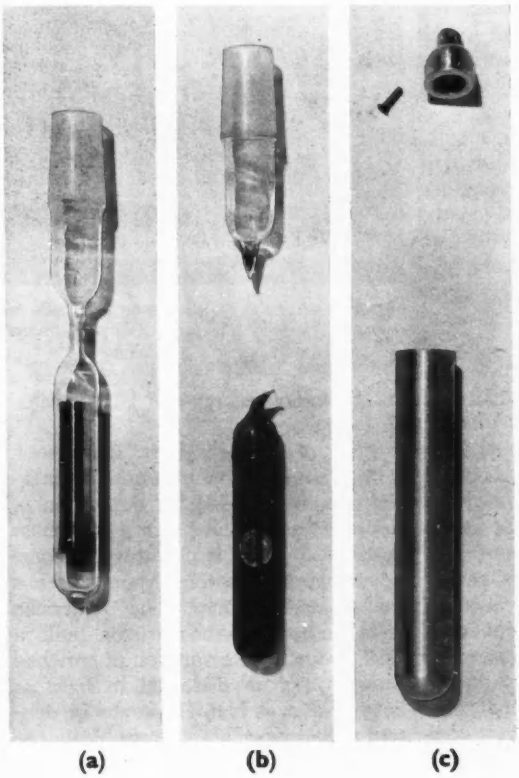

FIG. 2. Capsule for the irradiation of methane with fission recoil fragments: (*a*) Silica capsule, containing platinum foil coated with a 1 μ layer of enriched uranium oxide, before filling with methane; (*b*) the irradiated capsule is darkened by radiation. The disc is a gold foil neutron flux monitor; (*c*) an outer aluminium container is used in case the capsule breaks during irradiation in the reactor.

that if it were possible to achieve criticality with a natural uranium fuel, some 400 acres of plates would be required. Even with enriched fuel, the plate area would be troublesome and the added problems of supporting the plates and operating the reactor would lead to a high capital cost, to be charged against the chemicals produced.

Films of U_3O_8 (enriched in U^{235}) on platinum foil have been used by Dawson at Harwell and more recently by the author at Imperial College (Figs. 1, 2) and have proved valuable in research studies of fission fragment induced reactions.

CIRCULATING DUST REACTOR

A feasibility study of a chemonuclear reactor, in which the fuel is a dust of UO_3 particles (about 4–10 μ diameter) which is in contact with the reactant gases, has recently been described.[2] It is estimated that for a fuel density of 0·003–0·020 gm./c.c., it is possible to deposit 50–70% of the fission recoil energy in the gas stream (Fig. 3). Because of the small UO_3 particle size and the high velocity of the gases, however, the fuel could not remain in fluidised suspension in the reactor core but would be distributed through the reactor primary cooling circuit.

A reactor of this type would require a large quantity of fuel, because it is distributed throughout the gas circuits, and this would undoubtedly add to the capital cost. It may be possible to remove the fuel particles from the process gas in cyclones within the reactor reflector before circulating the gases to the external heat-exchanger.

Another type of fuel suggested by Harteck and Dondes,[3] who first demonstrated the value of using fission fragments for promoting chemical reactions, is a 1–5 μ diameter glass fibre incorporating up to 50% by weight of uranium-235. As the range of the fission fragments in this glass is about 7 μ, more than 50% of the fission energy would be released outside the fibre.

Cockelberg[4] has carried out considerable work using a dispersion of uranium oxide in microporous materials such as active charcoal, alumina, and silica gels. Under best conditions he has reported that up to 50% of the fission energy can be deposited in a gas system.

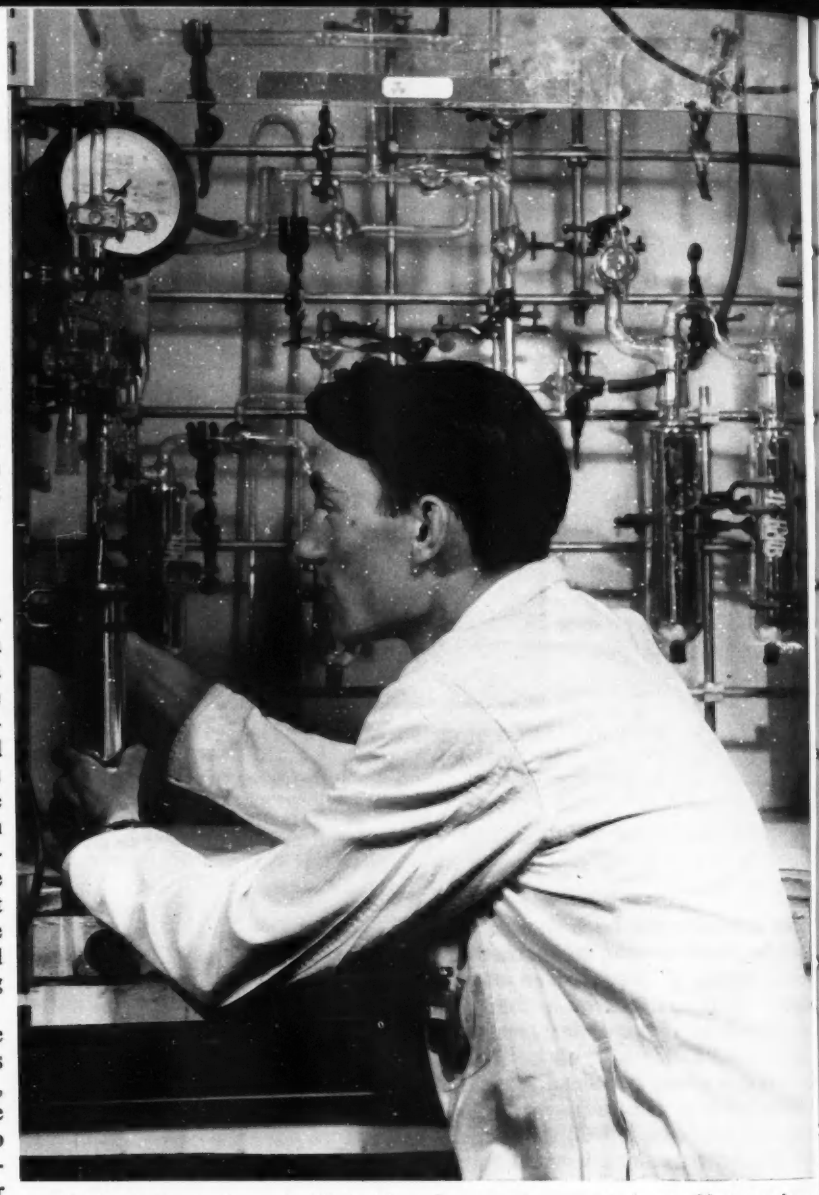

FIG. 3. *The silica capsule can be filled with methane gas at pressures of up to 30 atmospheres using a liquefaction technique, in this apparatus.*

Each of the fuels so far considered, with the exception of uranium hexafluoride, is intended for deposition of fission fragment energy in a two-phase system. There is, however, some advantage in choosing a homogeneous fuel-reactant system. The American aqueous homogeneous reactor, built at Oak Ridge, uses a solution of enriched uranyl sulphate dissolved in light or heavy water as fuel. Hydrogen or deuterium and oxygen are produced in the reactor core, as a result of the action of fission fragments and other radiations on water and, in fact, precautions are taken to recombine the gases in the reactor core. In this case the uranium atom is surrounded by water molecules and the complete fission fragment kinetic energy is given up to the water.

To make maximum use of this energy it would be necessary to use the chemical reactant as solvent for the fuel. As this solvent must also act as moderator and coolant, the range of chemical reactants is severely limited. If, on the other hand, the chemical reactants were dissolved in the liquid

fuel, once again the problem of non-productive absorption of energy would arise. The solvent might then absorb a large proportion of the energy.

QUOTE

Nitric acid and ethylene glycol are possible starters in the race for chemical manufacture using the direct energy from nuclear fission.

FURTHER FACTORS OF REACTOR DESIGN

Consideration must be given to the possible effect of neutron capture by the chemical reactants on the neutron economy of the reactor core. Fortunately for many elements in common use in chemical industry (for example, carbon, hydrogen, oxygen, nitrogen, fluorine, and sulphur), this is not too serious.

A greater difficulty arises because an essential part of the process is the ejection of fission fragments into the chemical reactants. This results in radioactive contamination through the fission products themselves, and also to possible neutron activation by the reactor neutrons.

It will always be necessary to decontaminate the chemical products from such radioactivity and the tolerance levels for contamination will be set at an extremely low level. This will be especially so if the product is likely to become associated with food stuffs.

Adequate decontamination will usually be possible by distillation, but even so the fact that a chemical product has been manufactured in a reactor may lead to public prejudice.

RADIATION YIELD

In radiation chemistry the yield of a product produced by radiation is expressed in terms of the G-value for the particular reaction. This is defined as the number of molecules formed for each 100 eV of energy absorbed. Yields of 10,000 or more can be obtained in some chain reactions (for example, in polymerisation), but in reactions not involving chain mechanisms the G-values are low, for example, usually between 0·1 and 10.

The fixation of atmospheric nitrogen to produce nitrogen dioxide (NO_2) occurs with a yield of about unity at atmospheric pressure, but, as we shall see, it is possible to increase the yield to $G=5$ or greater at higher temperatures and pressures.

CHOICE OF CHEMICAL REACTANT

The main factors to be considered in choosing a suitable process are:

1. The radiation yield of product should be as high as possible. The cost will be directly proportioned to the G-value.

2. The cost of raw materials must be low compared to the value of the product. This is of particular importance as the capital cost of the chemo-nuclear reactor is very great and, unless the reactor is also to produce electricity, the complete capital cost must be borne by the product.

3. The reacting atoms must have low thermal neutron capture cross-sections. This will ensure maximum neutron economy in the reactor, so reducing the quantities of expensive fissile material

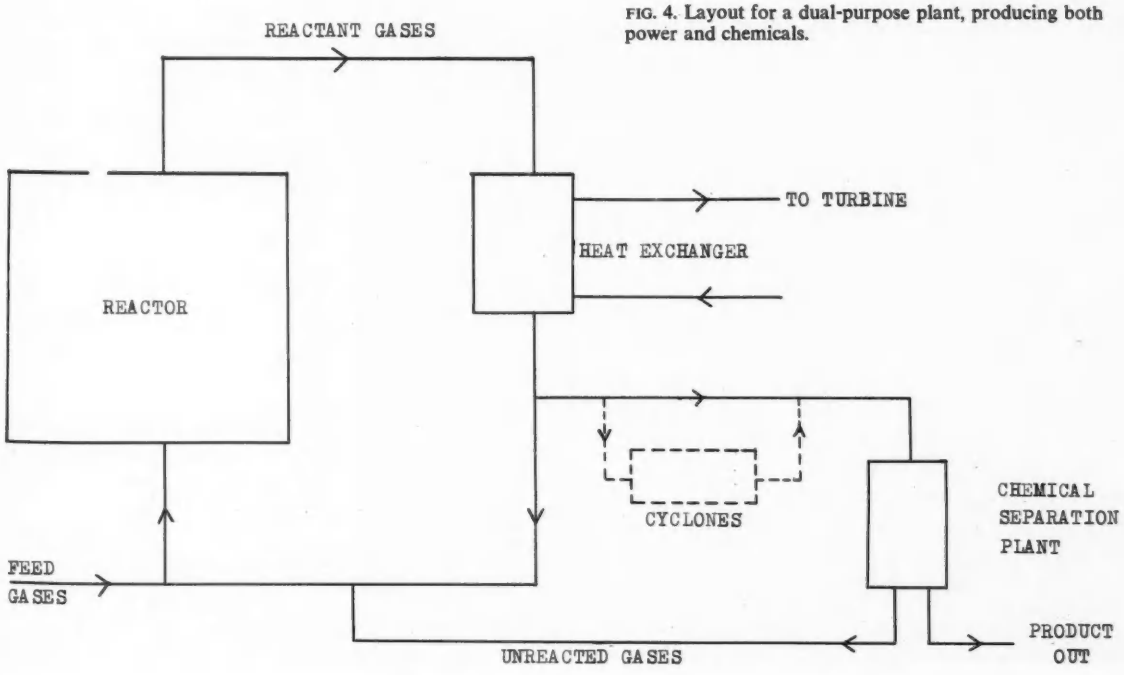

FIG. 4. Layout for a dual-purpose plant, producing both power and chemicals.

required, and also reducing the amounts of radioactivity in the product.

NITRIC ACID PRODUCTION

Only a limited number of chemical reactions have so far been investigated and these include both gas-solid and liquid-solid reactant-fuel systems. The most fully investigated reaction is undoubtedly the fixation of nitrogen. This has been studied under a wide range of conditions and, whilst the actual series of reactions which occur are very complicated and are not completely understood, it is possible to postulate reactions to explain many of the results.

Harteck and Dondes[3] have extensively investigated the fixation of nitrogen and have recently extended their static irradiation experiments to a study of flow systems. The source of fission fragments is a pad of uranium glass fibres installed in a reactor "loop". Mixtures of nitrogen and oxygen can be circulated through the loop at temperatures of up to 250°C and at a total pressure of 10 atmospheres.

In order to favour the overall production of NO_2, it is necessary to keep a low stationary NO_2 concentration, and this is achieved by leaching or freezing out.

Some of the many reactions occurring in this system are shown in Table 2.

TABLE 2. NITROGEN-OXYGEN REACTIONS

(1)	$N_2 \longrightarrow N+N$
(2)	$O_2 \longrightarrow O+O$
(3)	$O_2+N \longrightarrow NO+O$
(4)	$2NO+O_2 \longrightarrow 2NO_2$
(5)	$N+NO_2 \longrightarrow 2NO$
(6)	$N+NO_2 \longrightarrow N_2+O_2$
(7)	$N+NO_2 \longrightarrow N_2O+O$
(8)	$N_2O \longrightarrow NO+N$
(9)	$2N_2O \longrightarrow 2N_2+O_2$
(10)	$N+NO \longrightarrow N_2+O$

Equation 4 indicates that production of NO_2 is favoured by high oxygen pressures and low partial pressures of the product itself. In addition, at low pressures the NO concentration is relatively high. The formation of NO_2 by equation 4 is a third-order reaction, whereas N atoms react very rapidly with NO molecules by the reaction 10.

Harteck has already obtained a *G*-value of 5 for nitrogen fixation and hopes to achieve better yields of NO_2 when the kinetics and radiation yields for the various reactions are better understood.

Several calculations have been reported on the economics of this process. It would seem that, based on present results, a reactor built solely for nitric acid production is unlikely to compete with present-day commercial methods. Even if the more optimistic calculations are accepted, production costs lie close to present market prices. If chemical manufacturers are to accept such a novel process, they will only do so when production costs are considerably lower than those obtained in conventional processes.

The only way to do this in the case of nitric acid is to discover a method of producing NO_2 at considerably greater yields and to get a higher proportion of the fission recoil energy into the gases. A more likely possibility is a dual-purpose reactor.

In this, a major portion of the fission energy would be used to produce heat and hence power, but chemical production would be a valuable by-product. Fig. 4 shows a schematic diagram of the layout of such a plant.

OTHER REACTIONS

Because of its very high cost and possible large-scale use as a rocket fuel, some thought has been given to the formation of hydrazine by direct reaction between nitrogen and hydrogen. Irradiations using Van de Graaff electrons have resulted in very small yields of hydrazine at room temperature and *G*-values of about 0·1 at liquid nitrogen temperatures. Unfortunately, in the reversible reaction between N_2 and H_2 the dissociation of the product hydrazine increases as the temperature rises, and in the range of temperatures of interest in a reactor, decomposition will be rapid.

Other gas phase reactions which have been considered include nitrogen and hydrogen to give ammonia, oxidation of methane and other reactions with methane, and the irradiation of carbon dioxide to give carbon mon-

The Energy of Fission

When an atom of uranium-235 undergoes thermal fission, two fission fragments and an average of 2·47 neutrons (n) are produced. A typical fission reaction might be the following:

$$_{92}U^{235}+n \longrightarrow {}_{54}Xe^{144}+{}_{38}Sr^{89}+3n+192\ MeV$$

The fission products, in this example Xenon-144 and Strontium-89, occur over a wide range of the periodic table (from atomic masses 72 to 160), but the maximum yields are found around masses 100 and 135. Most of the fission products are radioactive and decay by beta or beta-gamma emission. The fission reaction is strongly exothermic, and the following Table 1 shows the various ways in which energy is released.

TABLE 1. DISTRIBUTION OF ENERGY IN FISSION

Kinetic energy of fission fragments	162 MeV
Kinetic energy of neutrons	5 MeV
Instantaneous gamma energy	5 MeV
Decay gamma energy	6 MeV
Neutron capture gamma energy	10 MeV
Beta radiation energy	5 MeV

oxide and oxygen. The latter is of interest because burning of the two products would subsequently release some 20% of the "absorbed" fission energy.

Patents[5] taken out by the American Hercules Powder Company describe a number of organic chemical syntheses performed in liquid and gaseous phases. The reaction which has apparently received most attention is the fission fragment irradiation of methanol vapour.[6] Irradiations, using thin films of uranium-235 on aluminium as the source of fissile material, were carried out at temperatures of 150° and 250° (Fig. 5).

Radiation yields for production of ethylene glycol from methanol ranged from 2–5 and best yields were attained in the presence of a small amount of carbon tetrachloride.

The reaction mechanism apparently involves the combination of $\cdot CH_2OH$ radicals which arise either when H· is ejected from a neutralised parent ion CH_3OH^+ or when a free radical (H·, CH_3·, CH_2·, etc.) extracts hydrogen from methanol; for example:

$$CH_3OH + \begin{bmatrix} H\cdot \\ CH_3 \end{bmatrix} \longrightarrow \cdot CH_2OH + \begin{Bmatrix} H_2 \\ CH_4 \end{Bmatrix}$$

$$2\cdot CH_2OH \longrightarrow (CH_2OH)_2$$

The economics of a process based on this reaction are claimed to be competitive with those for the present commercial manufacturing methods, although no figures are given. Ethylene glycol is used in large quantities as an anti-freeze and its selling cost is about three times greater than the selling cost of methanol.

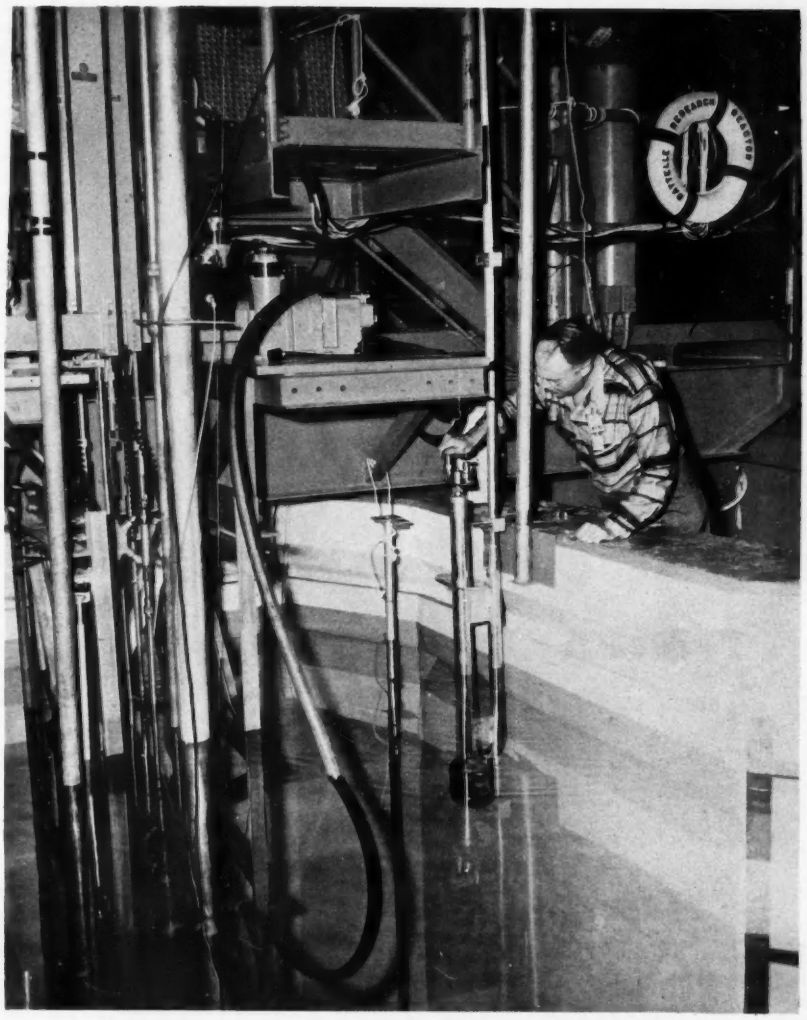

FIG. 5. Irradiation cell for studying the production of ethylene glycol by fission fragment reactions in methanol vapour. The cell is suspended close to the core of the Battelle pool reactor. (Photo by courtesy of the Hercules Powder Company and the Battelle Memorial Institute.)

FUTURE DEVELOPMENTS

As we have seen, the possibility of directly using fission energy in the chemical industry is a fair way off. Many of the reactions which have been looked at are not thoroughly understood and it appears that a long programme of basic studies is required. This should include not only a search for new chemical reactions, but also studies of the technology of plate and circulating dust fuels.

No really promising process has yet been found. Until one is discovered in which (*a*) the product is expensive by conventional methods of production, relative to the cost of raw materials, (*b*) the product is required in very large quantities, and (*c*) its *G*-value for formation is sufficiently great, it is unlikely that a chemo-nuclear reactor of the type we have discussed will be built.

At present it would appear that, if the yields for NO_2 could be increased, nitric acid might be the first reactor-produced chemical. Economic considerations[1] may lead, however, to construction of a dual-purpose reactor rather than one to produce chemicals only.

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[4] Cockelberg, R. F. R., "U.N. Conference on the Peaceful Uses of Atomic Energy", 1958, vol. 29, pp. 424–32.

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Right: Head of Lanthonotus *alive. Earth and clay readily adhere to the skin nodules and make the lizard very difficult to distinguish.*

Page opposite: Mr Harrisson (seated) with staff, sorting specimens for Chicago Natural History Museum joint Niah project.

THE EARLESS MONITOR LIZARD

(*Lanthonotus borneensis*)

TOM HARRISSON

Here at last is a live specimen of a lizard which apparently played an important role in the direct evolution of the snake. Sound has no impact on it, and it can "close down", reducing metabolism to an almost imperceptible minimum.

The mere sight of one of these alive and kicking would be the fulfilment of a dream no student of herpetology has ever realised.

These are the words of Clifford H. Pope in his authoritative "The Reptile World" (New York, 1957), when writing of *Lanthonotus borneensis*, Steindachner, the Bornean Earless Monitor.

Moreover, in their comprehensive "Living Reptiles of the World" (London, 1957), Dr Karl Schmidt and Dr Robert Inger write: *Few of the rare reptiles have been surrounded with the romantic aura of the foot-long Bornean lizard, the earless monitor! We are somewhat embarrassed to have to admit that on two separate expeditions to Borneo, we have failed to collect it or even to meet a native who had seen it.*

Lanthonotus was first described in 1878; since then fewer than ten specimens have been collected, all in Sarawak and none with proper find data. Its habitat and way of life have remained unknown, whereas its taxonomy, based on the spirit specimens, has received very detailed attention. This is one of the two genera of the critical lizard family *Helodermatidae*, the other genus containing the well-known Gila Monster (*Heloderma suspectum*) and the Mexican Beaded lizard (*H. horridum*), both venomous. It has been generally accepted that this family played an important role in the direct evolution of the snakes. (The definitive study is by S. B. McDowell and Charles M. Bogert in *Bull. Am. Mus. Nat. Hist.*, 105, 1954: 1–142).

For years, therefore, search has been made for a living *Lanthonotus*. Following failures to locate any, the Chicago Natural History Museum has assisted the Sarawak Museum to carry through an extensive study of Bornean reptiles, still continuing—and always with an eye alert for this missing lizard. For years now our well-attended Museum building in Kuching (Sarawak's capital)

Mr Harrisson is Curator of Sarawak Museum.

has displayed a picture of *Lanthonotus* and explanatory information in Malay, Chinese, Dayak and English, offering a good reward for information leading to the capture of one—with no result.

But in January 1961, one of our full-time collectors, Jommel bin Bogol, working on this reptile project around the Niah Caves—where the Museum has a permanent research station—heard of a strange lizard reported by Dayaks nearby. From a small hole some half a mile from the Niah River, a thirteen-inch living *Lanthonotus* was at last obtained.

Brought the 300 miles to Kuching in a tin of earth, it at first refused any sort of food—including the baby mice advised by cable from Dr R. Inger, present Curator at Chicago. We kept the lizard in our bedroom (air-conditioned). After many abortive trials, it suddenly accepted the raw yolk (only) of marine Green Turtle (*Chelonia mydas*) eggs, which are fortunately marketed through me as Curator, so that I can get a supply even during the monsoon. Through February and March it took this and a little water nightly. It *could* move about by day, but rarely did so. Scent was more important than sight. Sound seemed to have no impact whatever, even if near and intense.

Dayak finder of the lizard, hoeing at the spot of find—ten inches down in garden clay.

IT PREFERRED MY WIFE

At night it would come out, from a deep, dark box, for short periods only. Any form of activity seldom lasted more than five minutes. The liquid yolk was tested by licking, then sucked in. The teeth were never seen to be used in any way. Handled, it showed no inclination to bite; indeed, soon became extremely tractable and "tame"—much in the same way as the Gila Monster, sometimes kept as a pet in America. It could distinguish between my wife's handling and mine. It much preferred hers.

It enjoyed a daily spell in the wash-basin, was fully aquatic, swimming gracefully and staying submerged for long periods—although the hole where it was found stood 100 ft. from any water. The fore limbs, mainly used in swimming and walking, were nevertheless very weak; strength in general was vested in the neck and forward trunk. Many of the movements were in effect those of a snake with ancillary legs. Only the snout seemed useful in burrowing. It is likely that holes of other animals are borrowed—I think.

For hours—and sometimes days—at a time, however, this lizard simply lay in a crumpled-up state, which in any other animal would be taken as "death". It had the capacity, both on land and in the water, to "close down", to suspend or reduce essential metabolism to an almost imperceptible minimum.

At the end of March, we transferred from our Kuching home back to Niah Caves and took the lizard with us, since there was no one else able to spend the time to care for it. (If not bathed, it got too dry. If too damp, it rapidly developed eye and nostril fungal infection.) Turtle eggs cannot reach Niah, but by now it accepted fresh raw hen's eggs, also. Unfortunately, however, on April 11, within a mile of its own home, it suddenly died. It took some time to be sure it was dead.

A full day and night account of this *Lanthonotus* was kept and its actions filmed. It is clearly an extraordinary animal. In our decade of experience keeping Borneo reptiles, we have seen nothing to compare with such lethargy, yet specialised activity. Much remains to be learned, and energetic search for new live specimens is now in hand at Niah. A report just received from far up the Niah River rumours a much larger example killed there recently. The Gila Monster goes to 3 ft., and presumably *Lanthonotus* could do the same. The passive, nocturnal, and subterrestrial habit could readily account for it having been overlooked, despite world-wide interest in the species for over half a century.

On the other hand, the place where this one was found is of a ground and floral habitat repeated over many thousands of square miles. It lay collapsed, in a cavity ten inches down in thick clay, below the top soil, at the foot of a knoll, in a band of secondary scrub lately used for rice-planting and soon due to be used again. It was accidentally exposed by an Iban Dayak while cutting slightly into the knoll to improve the path for his new pepper garden just behind. The whole area is densely cultivated by Dayaks and

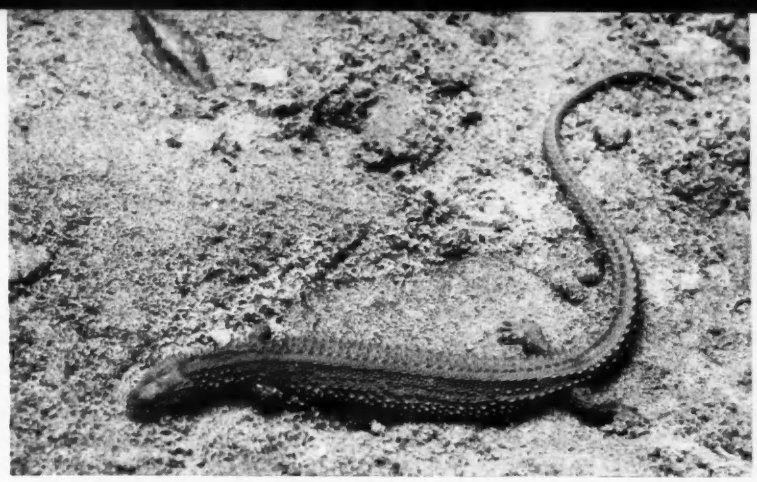
Chinese. There is no primary jungle in sight.

To my eye, there is *nothing* special or peculiar to this habitat. I am inclined to think that in fact *Lanthonotus* may be "common" over the subcoastal zone of West Borneo. In that case, it has simply escaped detection, aided by a capacity to retain soil and debris between the nodular skin tubercles, acting as effective camouflage. Borneo is a country of which we know so little scientifically that it remains unsafe to say *anything* is rare until much more work has been done over a wide area. It would therefore be no surprise if *Lanthonotus* suddenly turned up all over the place.

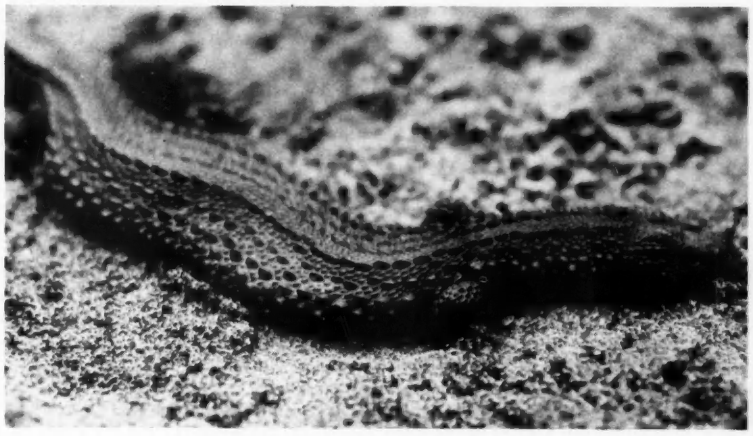
MISREPRESENTED MONSTER

In this connexion, it is now clear why we got no response to our Museum appeal over the years. We displayed Steindachner's original illustration (reproduced by McDowell and Bogert, *op. cit.*), which is the only one purporting to show the live animal. The drawing looks *nothing* like a real live *Lanthonotus*. It represents a miniature but vigorous "monster", with a pushed-up under-lip and a hungry, if crafty, twinkle in its tiny, piggy eye. This was influenced, no doubt, by the relationship with the Gila Monster, which does look *tough*. The Borneo Earless Monitor looks, and is, one of the most untough animals imaginable.

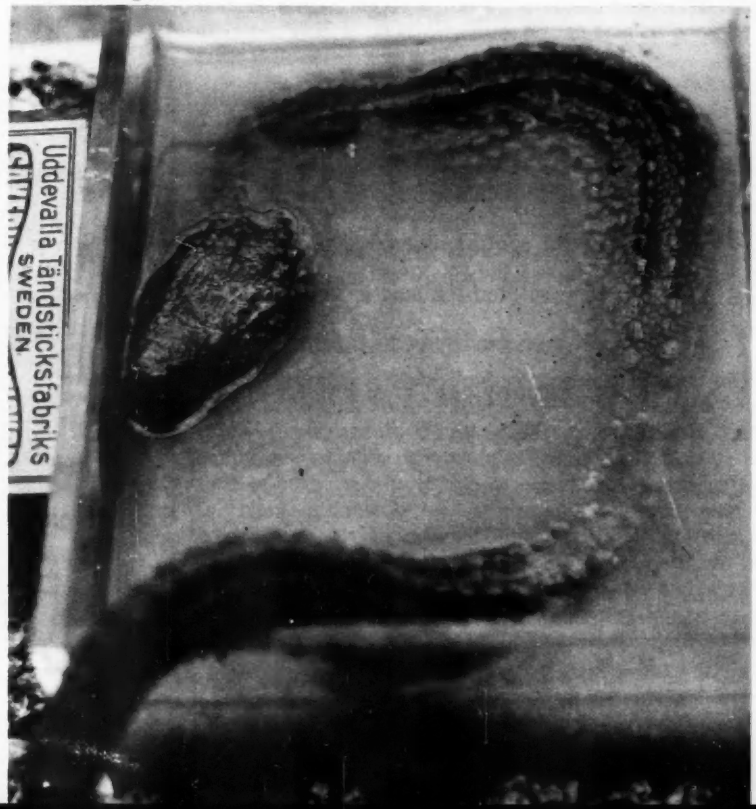
It could be, of course, that our pet was unnaturally pliable. But it is difficult to imagine under any conditions this little lizard bursting with vim or deploying any kind of cunning. On the contrary, it may well owe its capacity to survive in a densely populated agricultural zone like Niah largely to a guileless, placid unvim. In this it resembles some of the little-known ground snakes also found in West Borneo, though these are always more easily spotted by eye and more active—at least when disturbed. You could not really disturb a *Lanthonotus*, if it was not in the mood. Certainly you could never have it "alive and *kicking*", in Dr Clifford Pope's phrase (above)! A hundred could kick me at once, and I would not mind. Let us hope we find many more, and can give them a chance to show their queer form to zoologists more widely. . . .



The lizard alert (below) and collapsed (above). Note the tiny eye and even smaller nostril.



Breathing position, half-submerged. Scent means more to it than sight.



THE PROSPECTS FOR COMPUTER CONTROL IN INDUSTRY

F. R. HIMSWORTH

Feed-back control of automatic plant is well known. So is the simulation of operating conditions to produce specific examples of feed-forward control. But recent experience has isolated the sort of conditions in which feed-forward control can be continuously applied to process plant.

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The operation of a large and complex plant involves heavy computation, and so it should benefit from the use of a high-speed computer. But just what are the conditions in which a computer can be continuously applied to the control of a plant? This has been the subject of several investigations, and the following outlines some of the general principles involved.

The capacity of a computer for carrying out complex calculations very quickly can be of great assistance in the understanding of processes, in the design of plants, and in the investigation of methods of operation. But if a computer is to be used as a direct aid to control of a plant, more is required than arithmetical operations. It must be able to take certain decisions; in particular, it must be able to recognise when the plant is not behaving "correctly" and to inform the manager that this is so.

A digital computer, properly programmed, can do things of this kind and, if it is to be successfully used for process control, all its facilities must be used to the maximum. It is almost true to say that a computer, given adequate instructions, can do anything that a plant manager can do in controlling a process. But this alone would not justify its use. We have not reached the stage at which the plant manager can be dispensed with and control handed over completely to a computer; we probably never shall.

In other words, a computer must not be regarded simply as an automatic device for doing what is already done manually. On this basis it could not be justified, since on most modern chemical plants there is almost no possibility of saving labour or management costs. They are already so highly instrumented, with automatic controllers at every

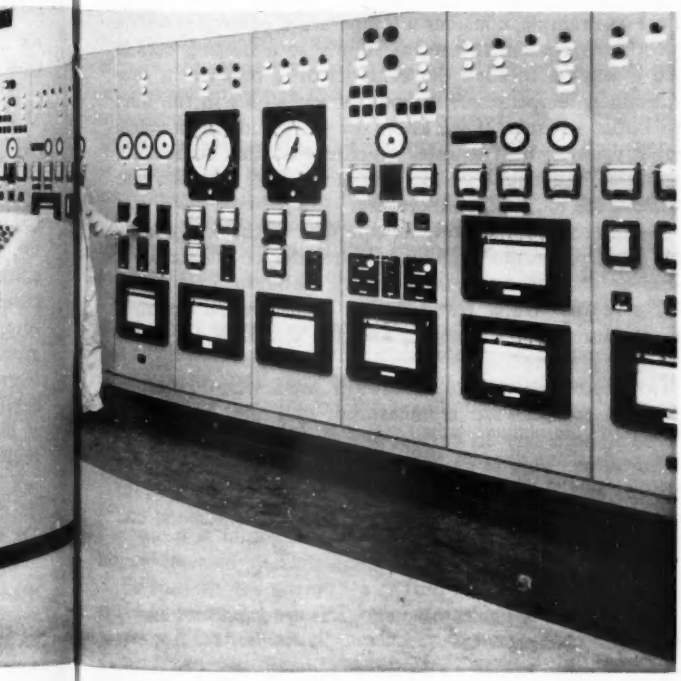
FIG. 1. *Control Room, Oil Gasification Plant.*

stage, that one or two men can operate a large plant. It is not uncommon to find that one man is there in case of emergencies and a second in case something happens to the first. Fig. 1 shows the control room of a large plant. The plant is controlled entirely from here, except for emergencies and perhaps in starting up.

The computer can justify itself only if it achieves better results than are possible with manual control; for example, steadier running, higher output, better product, lower costs. To achieve these objects we must associate with the computer the most efficient means of effecting control, which will usually involve computations that cannot be done manually in the time available. But we must also retain as far as possible those features of control by plant manager which are more than calculations—decision making, detection of abnormal operating conditions, and so on.

In particular, the computer must be able to detect a sudden emergency, say a leak of inflammable gas which might cause a fire if not quickly checked. It must then either cause appropriate action to be taken or, if that is impracticable, operate an alarm. The appropriate action may be complex and widespread; if, say, a main cooling water-pump failed, dozens of units might have to be shut down and restarted when the supply was restored. These are critical operations and in many cases may require human controllers.

It will be helpful to have a simple example which can be used for illustrations of various points. Fig. 2 is a very much simplified outline of one stage in the manufacture of ammonia. A and B are two streams of gas, each containing hydrogen and nitrogen, but in different proportions. The two are mixed in a ratio which is determined by the settings of the valves V_A and V_B. The mixed gas, M, enters the converter where part of it—about 20%—is converted to ammonia. The remainder, after removal of the ammonia, is recycled to join M and re-enter the converter.



Since A and B contain small amounts of other gases, mainly argon which cannot readily be removed from atmospheric nitrogen, some gas must be purged to remove these "inert" gases. If M contained hydrogen and nitrogen in a ratio of exactly 3 : 1, the gas at all subsequent points would have the same ratio (since ammonia is NH_3). If M contains a small excess of hydrogen, there is a steady build-up of hydrogen in the recycle system and in the purge gas until the rate of removal of hydrogen and nitrogen in the ammonia and in purge just balances the rate at which they enter in M.

Since the purge rate is very small compared with the make-up rate, the ratio of hydrogen to nitrogen in the purge, and hence in the recycle gas and in the gas entering the converter, differs from 3 : 1 much more than that in make-up gas. The consequences are that the purge rate, especially of hydrogen, is increased and the efficiency of the converters is reduced. The problem is therefore to maintain the ratio of hydrogen to nitrogen in M as close as possible to 3 : 1; only a very small error can be allowed to persist for any length of time.

Control of the composition of M can be accomplished in three ways:

1. The flow rates and compositions of gases A and B are measured. The relative rates which will give a 3 : 1 ratio are calculated, and the setting of V_A or V_B is adjusted accordingly. If the rate or composition of A or B changes, an adjustment is made.

2. The composition of M is measured and, if it is not correct, the setting of V_A or V_B is adjusted by a calculated amount. This is done continuously, so that as soon as an error is detected, action is taken to correct it.

3. The composition of the recycle gas is measured and, if it is not correct, that is, if the hydrogen : nitrogen ratio is not 3 : 1, V_A or V_B is adjusted by a calculated amount. This method is similar to (2), but the composition of the recycle gas is very sensitive to errors in M; that is errors are more easily detected.

Method (1) is an example of "feed-forward" control: (2) and (3) are "feed-back" control. These will be discussed later.

SIMULATION OF A PROCESS

If a computer is to be used to control the operation of a complex plant, it must be able to calculate the effects of any changes in operating conditions. If it takes a certain control action, say changing a reactor temperature, it must be able to predict the results of this action at all relevant parts of the plant. It must know these results before it takes the control action, otherwise it does not know whether the action is beneficial or not. The action must be taken only if it will improve process efficiency, and this implies that, for example, a dangerous condition is not caused at any point, maximum flow rates are not exceeded, etc.

QUOTE

A simulation of a process can take account of the control variables and measured variables, but none of the unmeasured variables.

If enough is known about the individual operations on a plant, each operation can be fully represented by a formula which gives, in numerical terms, the effect of the plant unit on the materials passing through it. The following are some simple illustrations:

(*a*) If two fluid streams meet and are mixed, the rate and composition of the combined stream are easily calculated from the rates and compositions of the two original streams.

(*b*) If a two-component liquid is distilled, the rates and compositions of the top and bottom products can be calculated if we know enough about the characteristics of the still. The heat required for distillation can also be calculated.

(*c*) If we know enough about the operation of a chemical reactor and are given the feed rates of the reacting materials, we can calculate the rates of output of the products of reaction. It is necessary to fix the relevant parameters of the reaction, such as temperature, pressure, etc.

If such formulae can be established for every unit in a plant, they can be linked together, giving a more or less complex set of equations which can be solved. Thus if we assume the feed rates of the raw materials and the temperature, pressure, etc., at all relevant parts of the plant (or such of them as can be determined beforehand) we can calculate the rates of output of the products. Other things such as purity of products, operating costs, etc., can be calculated from the same basic equations. This is a "simulation" of the process. If simple enough, the calculation can be carried out by hand; otherwise a computer may be required to produce the results in time. An important advantage of using a computer is that many alternative sets of operating conditions can be studied with little extra effort or cost.

A simulation can be used in many ways. It is useful in the design stage, even though operating data may be rather uncertain, in finding the most economical sizes and numbers of units. It can be used to assist management in deciding how best to modify conditions to meet changing circumstances. It can be used for the analysis of process operation, for example, where materials at intermediate stages are difficult to analyse and only product analyses are available. In general it may lead to a better understanding of the operation of the plant and suggest means of improving it.

Finally, it can be incorporated in a system of process control; it must in fact form the nucleus of any such system applied to a complex plant.

There can be no doubt that to prepare a simulation of a process is well worth while. It is seldom possible to say what precise benefits will be obtained, but the very act of preparing the simulation is likely to focus attention on the inadequacy of the quantitative knowledge about some operations. When the simulation has been built up into an adequate representation of the process, including costs, it is a most useful tool for investigating causes of inefficiency, correct balance of plant units, optimum means of attaining different patterns of output, and so on. Such uses of a simulation may be described as "off-line" computing; the computer is not connected into the plant control system, but is used as an aid to design and management.

COMPUTER CONTROL

A further extension of the use of computers for process improvement would be to hand over to the computer the control functions now exercised by management; that is to use it for "on-line" computing with the aim of automatically keeping the plant running under optimum conditions. Here it is necessary to mention two points which will emerge later:

(i) It is unnecessary and indeed impossible for the computer to take over all the functions of the foreman or manager.

(ii) It is not sufficient for the computer to do just what management can do; it must help management to do better to justify its use.

Before discussing the possibilities of computer control we must decide what we mean by "control".

FEED-FORWARD AND FEED-BACK CONTROL

The automatic controllers now widely used to control individual process variables are all of the "feed-back" or "closed-loop" type. "Feed-forward" control is more difficult. For example, consider a temperature controller whose object is to control the steam supply to a heating coil in such a way as to keep the contents of a vessel at a predetermined temperature, in spite of variations in the inlet and outlet streams. If we knew the inlet and outlet rates, heat capacities, heat transfer co-efficients, etc., sufficiently accurately, we could predict the steam rate necessary to maintain the required temperature.

But information is rarely as complete as this and in any case some properties such as heat transfer co-efficients are liable to change. So even this very elaborate calculation may give the wrong result. Such "feed-forward" control can at best be approximate, and for chemical processes is seldom good enough.

The feed-back system is always used when possible. Here the temperature is measured, and the steam rate altered if necessary to correct any deviation from the required value. This is more certain in its results, easier to operate, and independent of such things as changes in steam pressure, changes in heat transfer co-efficients, etc.

QUOTE

If changes due to unmeasured variables are important and occur frequently, manual control may be required more or less continuously.

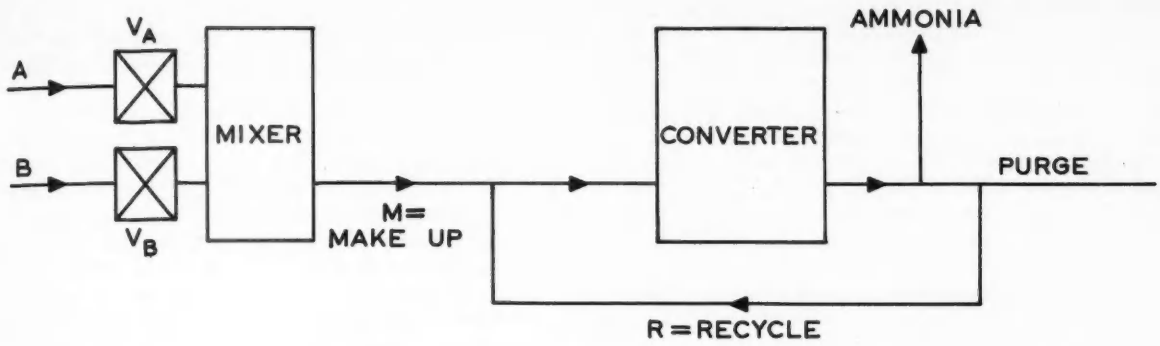


FIG. 2. *Outline of Ammonia Plant.*

It overcomes any inevitable ignorance of some features of the system; all we need know in fact is that the more steam we supply, the higher will be the temperature, although the engineer does use much more information in designing a good controller.

Referring to the example of the ammonia plant, method (1) represents feed-forward control. The rates and compositions at the start of the process are measured and one or more of them is adjusted so that the calculated composition of M, and hence of R, is correct. The advantage of this method is that the adjustments are made before the gases enter the mixer; if the measurements are accurate and rapid, and if the control action is carried out accurately, the composition of M can in principle be maintained exactly constant. The disadvantage is that if the measurements or the control action are not correct, the composition of M is wrong, and this information is not used in the control system.

Methods (2) and (3) are feed-back systems. The measurement is not made until after mixing; it is used to make an adjustment earlier in the system, that is, the result of the measurement is fed back to the control valve. The disadvantage compared with feed-forward control is that there is a delay, or time lag, between detecting an error and correcting it. All the gas between the control valve and the measuring instrument has the wrong composition before this can be known. It is thus impossible to keep the composition constant if A or B is varying. The advantage is that control is based on an actual measurement of the make-up or recycle gas composition. If the control valve is not accurately set, this will be detected, and further adjustments made until it is correct. The effectiveness of feed-back control depends on the length of the delay between the control valve and the measuring instrument. Method (3) is clearly inferior to (2) in this respect, but it has the compensating advantage that the measurement is more sensitive. It may be advantageous to tolerate the long delay because it is impossible to detect small errors which could not be detected by measurements on M.

To sum up, the advantage of feed-forward control is that there is no delay in making a correction. The disadvantage is that it relies heavily on accurate measurement and control settings. Feed-back control has the disadvantage that it involves a time delay, that is, it always makes its corrections too late. Its great advantage is that it relies only on measurement of the final product so that, provided the measuring instrument is accurate, it detects and adjusts for any error, however caused.

We must distinguish carefully between feed-back and feed-forward control, especially as "automatic control" is now almost synonymous with "feed-back automatic control". Computer control might be imagined to be a superior, more complex form of automatic control, and therefore feed-back control. Unfortunately it cannot be, except in a very modified sense of the term.

A simulation could readily be adapted to feed-forward control. It would calculate suitable, or even optimum, values for all the process variables for a given raw material supply and a given output of product. It is thus asked to predict what the output will be when the plant is adjusted according to the calculations, just as the feed-forward temperature controller predicts temperature when the steam rate is set at the calculated value. The drawbacks of imperfect knowledge, unknown changes, etc., still apply, and if the product does not conform to the prediction the manager must either tolerate what the plant produces (if it is tolerable) or take some other action to put matters right. In other words he will close the loop himself.

Can the computer be used to close the loop? Not directly; it can carry out all the instructions given to it but it would hardly be possible, even in principle, to build into it the whole of the manager's and foreman's knowledge of the plant. It can carry out complicated calculations, and can compare the performance of the plant with that predicted by the simulation. If the achieved performance is not so good as expected, it can follow some logical sequence of tests to find out why this is so, and then either (*a*) take some action which might improve matters or (*b*) identify the section of plant which is causing the trouble and inform the manager of this finding, say by sounding an alarm and printing out the source of trouble, or (*c*) simply inform the manager that the trouble exists if it cannot locate the source.

Thus, while a computer cannot close the loop in the same positive way as an automatic controller, it can go some way in this direction and, because of its logical

capabilities, it can when necessary hand over control to the manager.

There is another, equally important difference between automatic control of a variable and control of a whole plant. A temperature control is set to maintain a given temperature. If the actual temperature is too high (or too low) the control reduces (or increases) the steam supply to correct the error. But a process should be operated, not to produce a fixed output or a fixed rate of profit, but to produce a maximum rate of profit or maximum efficiency. Its target is not something definite, but a maximum.

If it is operating at the maximum and something happens which affects its efficiency, the only possible result is a fall in efficiency. If all that we observe is the fall in efficiency then, even if we could guess which variable had caused the fall, we would not know even in which direction to make a change. The best we could do would be to try a change in one direction and, if this made matters worse, try a change in the other direction. This might have to be done for several variables before the real cause was found. Worse still, the fall might be due to some change over which no control is possible, and the maximum now attainable may be lower than that previously obtainable. For example, in hot weather warmer cooling water reduces the efficiency of many processes. Optimisation is a much more subtle process than maintaining a fixed value which it is known can always be attained.

These results apply to optimal control of a plant by any means and are not peculiar to computer control. But to make effective use of a computer the methods used for attaining and maintaining optimum should be as efficient as possible, and should take account of the power of a computer for rapid calculation and for the other logical processes of which it is capable.

TYPES OF PROCESS VARIABLE

Before discussing the problem further it is useful to consider the categories of variable which affect a process. They may be classified for this purpose under three headings:

(*a*) *Control Variables*

These are variables which can be controlled at will, by hand or automatically, at any desired value. The temperature, pressure and flow rate of a process fluid are typical examples.

(*b*) *Measured Variables*

These are variables which can be measured but cannot be controlled. They must be capable of being measured continuously, or at least with a relatively short delay, that is, the values must be available to the operator or computer in time to be used for controlling the plant. Their effects on the output must also be known, even if imperfectly. An example is the composition of a raw material which is not under the control of the plant manager, but which can be measured accurately and quickly enough.

(*c*) *Unmeasured Variables*

These are variables which may be unknown; or not capable of being measured; or if measurable, the results are not available in time to be used; or even if measurable their effects on plant performance are not known. The activity of a catalyst or the heat transfer co-efficient of a heat exchanger may change with time and it may be impracticable to measure them. Leaks or imperfectly acting valves are things which cannot even be measured. Instrumental errors can be measured, but usually persist for some time before being revealed by routine checking.

A simulation of a process can take account of the control and measured variables, but not of the unmeasured variables. If knowledge of the process were complete, that is, if there were no unmeasured variables and if the effects on the output of all the controlled and measured variables were known at least approximately, the simulation could be used for control. Into it would be fed the values of all the measured variables; it would then calculate the optimum settings for all the control variables, which would be reset either by hand or automatically.

This would constitute a form of computer control which is in a sense closed loop control, but it is not feed-back control, since the output plays no part in it. We have assumed that no unmeasured variables exist, so that feed-back is unnecessary and indeed redundant.

If there are some unmeasured variables, as there always must be, even if they are nothing more than mechanical faults, instrumental errors, etc., the predicted values of the output will not agree with observation. The computer can do nothing about this, since it has already been fed with all the available information. It can, however, detect when such a discrepancy occurs, and may also be able to identify the section of the plant which is responsible.

To take a simple example, if the control signal sent out were to increase the flow in a certain pipe, and if the increased flow did not take place, the computer would indicate this as something to be investigated; it could be a choked pipe, a sticking valve, failure of the valve positioning mechanism, etc. Such indication must be built into the computer programme; otherwise it may blindly try to do the impossible, making conditions worse than ever, instead of telling the manager immediately that something is wrong, which it can easily be programmed to do.

If the discrepancy is serious, the plant manager will do something about it. He might examine the plant for mechanical faults, recalibrate some key instruments, try the effect of adjusting a few of the important variables; for these he relies on his experience, and not on calculations. This situation is not a consequence of computer control, but it does indicate that it will be necessary at times for the manager to take charge. If changes due to unmeasured variables are important and occur frequently, manual control may be required more or less continuously. This does

QUOTE

By combining simulation with the cut-and-try method, feed-forward control can be used for rough adjustments towards the optimum, while feed-back control is used for fine adjustments.

QUOTE

The analysis of process records for control purposes is usually unrewarding.

not necessarily mean that a computer cannot help. Its effect in correcting for changes in the measured variables may be sufficient to justify its use; even though the unmeasured variables prevent it from achieving optimum results, it may still do better than manual control. If the main sources of variation are the unmeasured variables, optimisation must be done in some other way.

TRIAL AND ERROR METHODS

To attain and maintain optimum conditions in spite of changes due to unmeasured variables inevitably means some form of "cut and try" operation. Such a method has recently become familiar under the name "evolutionary operation", which is simply a systematic way of applying the cut and try principle with the object of keeping a process at near maximum efficiency. Groups of variables are changed by small amounts, and the effects of the changes observed. According as these changes are favourable or unfavourable in their effects, further changes are decided upon.

This process may either stop when the optimum is reached, or be continued as routine to ensure that optimum running is maintained. The entire process could easily be mechanised; a computer could be used to set the process variables, measure the effects on output, calculate the changes indicated by these results, re-set the process variables, and so on indefinitely. Whether this would be economically justified depends on many considerations, for example:

The time required for the plant to settle down after a change.

The speed at which the output variables can be measured.

The complexity of the calculations.

The frequency of disturbances which affect efficiency.

If it were necessary to run for some hours after every change before the effect could be adequately measured, or if it were to take a day to analyse the product, or if the calculations were very simple, or if the process were so stable that a check once a week was sufficient, there would clearly be no case for using a computer. Processes for which a computer would be necessary or justifiable are not likely to occur frequently, at least in the chemical industry, for this method of operation.

Simulation and evolutionary operation could be usefully combined under certain circumstances. If major changes in important variables occurred frequently, the approximate effects of these changes would be calculated using the simulation. The computer would then reset the control variables to their optimum values, as well as possible on the information available. Final optimisation would then be made by cut and try methods. This amounts to the use of feed-forward control for rough adjustments, and feed-back control for fine adjustment. It is a type of control system which deserves more attention, and one in which a computer could well play an important part.

The more perfect the simulation, the less the need for feed-back. In the beginning, with a very crude simulation, it would be necessary to analyse the operating data in order to detect the shortcomings of the simulation. The results of such an analysis could be fed back and used to improve the simulation. Thus by alternately using the simulation to predict the likely means of improving the process, and using the resulting process data to improve the accuracy of the simulation, it may be possible to progress towards a situation in which feed-forward control is highly effective and no attempt need be made to close the loop other than by indicating when, and if possible why, the predicted performance is not achieved.

These are very general statements which cannot be fully developed here and which in any case are difficult to follow up unless referred to a specific example.

ANALYSIS OF OPERATING RECORDS

Another use of computers which is relevant to process operation and control generally is the analysis of process records with the object of establishing equations for use in a simulation. Instruments and data loggers can produce enormous amounts of such records and computers can digest these readily. It is often assumed that since the process variables, including the output of the plant, must be connected by laws of some kind, and since the variables all show some variation, however slight, it must be possible, by analysing sufficient data, to arrive at these laws empirically. It has been suggested that the computer should carry out such an analysis continuously, using the currently determined form of equations to predict and optimise plant performance.

Unfortunately, and for very good reasons, the analysis of process records is usually an unrewarding activity. The analysis is using variations which the control system is trying to minimise; the better the control, the less certain the analysis. It is much more informative to make deliberate changes, observe their effects, and hence deduce the empirical relationships. This brings us back to evolutionary operation, and to an analysis of relatively few data by hand or by computer.

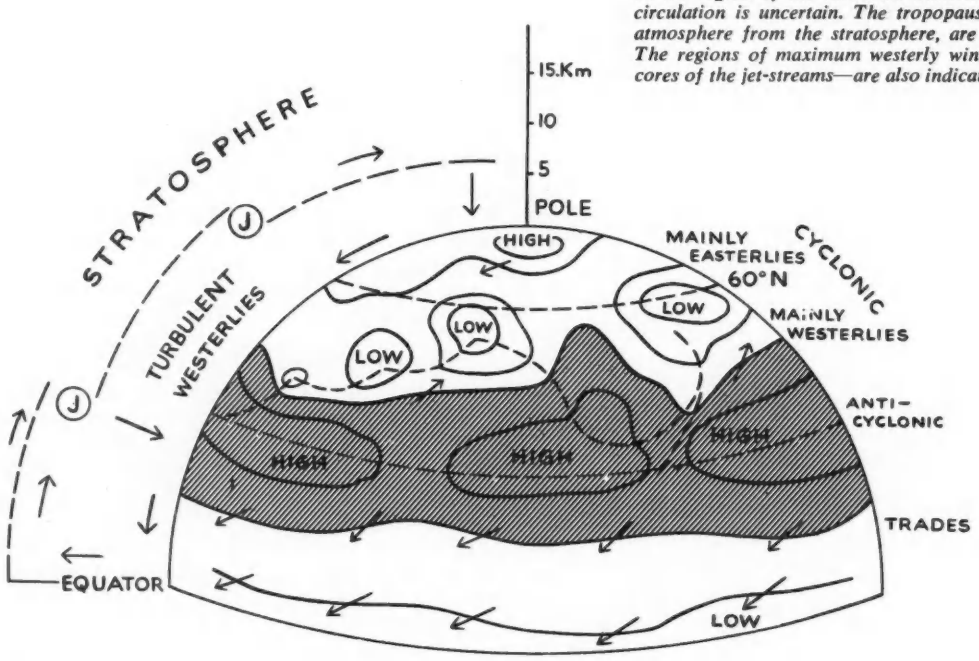
A plant simulation is simply an assembly of relationships, theoretical or empirical, and it may be that for some plant units the relationship changes with time. It is likely to retain its general form, but some parameters may change. The most obvious example is the decay of a catalyst; the form of the kinetic equations does not change but velocity constants often decrease more or less rapidly as the catalyst activity decreases.

Thus if it is desired to use a simulation in some form of feed-forward control it may be necessary to adjust it from time to time to take care of secular changes. A computer may well be useful for this kind of application, particularly if the simulation itself requires a computer. It could be programmed to accept records of plant operation, which could be either normal records or, better, the results of deliberate experimental changes in the important variables, carry out

Continued on page 320

FIG. 1. GENERAL CIRCULATION OF THE ATMOSPHERE

Schematic representation of the pressure distribution and the winds at the earth's surface of the northern hemisphere. On the left is a vertical section, vertical scale expanded about 100 times, showing the direction of mean circulation in the vertical plane. In the region of the turbulent westerlies the direction of mean circulation is uncertain. The tropopauses, separating the lower atmosphere from the stratosphere, are shown by broken lines. The regions of maximum westerly winds in the upper air—the cores of the jet-streams—are also indicated.



UNSOLVED PROBLEMS OF WEATHER AND CLIMATE

R. C. SUTCLIFFE

We have yet no basic theory of the general circulation, but there is every reason to believe that one can be evolved in a matter of years "if a sufficient number of able people can be wooed away from the blandishments of atomic nuclei and cosmic space".

The study of weather and climate is the science of meteorology, but not by any means the whole of that science. Meteorology is the study of the earth's atmosphere in all its aspects: its composition and its chemistry; its radioactivity, natural and man-made; the pollution of the air from houses and factories, and how this accumulates, travels and in time disperses; the transmission of sound through the air and the transmission of all kinds of electromagnetic radiation, from the ultraviolet, the visible and the infra-red through the spectrum to radio wavelengths; and the special phenomena of radar and radio reflections from clouds and precipitation.

The science of meteorology also includes the study of a multitude of optical effects—sky colours, haloes and coronae, scattering, refraction, diffraction, etc.; electrical phenomena—not only the spectacular lightning but the remarkable potential gradients and

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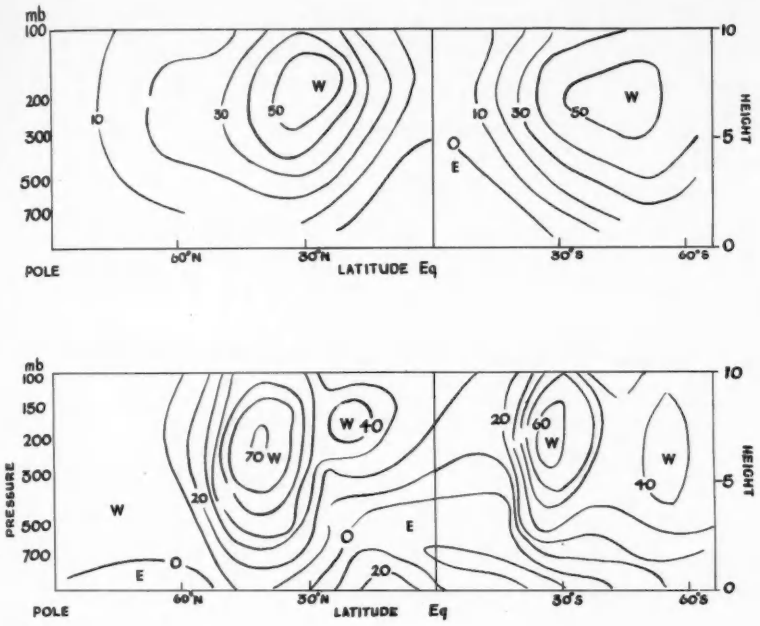


FIG. 2. *Actual north–south sections through the atmosphere showing the strength of the westerly winds and the jet-stream centres. Above is an average taken round the whole world. The lower diagram is the average for the month of April at 180° longitude, that is, across the Pacific Ocean. Two maxima are shown in each hemisphere.*

electric currents revealed by instruments in apparently quite tranquil weather; and, of course, the special behaviour of the high atmosphere—for example, the photochemical effects that produce a protective screen of ozone at about 25 km. overhead and all the ionisation, auroral, magnetic, and electromagnetic effects which have long been of deep concern to radio engineers and have now become the pursuit of those who make use of the new vehicles, rockets, and artificial satellites. As one goes up higher, meteorology finally merges into the realms of space science.

Since the atmosphere is continuous, with its physical properties so interlinked, most if not all of these more recondite parts of meteorology have a bearing on weather and climate, either directly in cause-and-effect relationships or indirectly as the means for obtaining useful information. There is thus no essential difference between the complete meteorologist and the complete weatherman, but I would like to give attention here to those problems which are obvious and important to every inhabitant of our planet.

Naturally I shall not catalogue all the meteorological fauna but shall pick out a few important animals which dominate the population. First, there is the general circulation of the earth's atmosphere as a whole, the winds and weather experienced on our planet on the broadest possible scale. Second, we must mention the monsoons, a word which in technical usage means the varying large-scale circulations from summer to winter or from season to season, not only in India but over much of the earth. Third will come the familiar depressions and anticyclones of our daily weather map; next the fierce cyclones or hurricanes of the tropical oceans which are responsible for untold damage and the grievous loss of life every year; and finally, showers and thunderstorms responsible for much heavy rain and destructive hail. A brief description of each of these as illuminated by recent research work will show where some of our greatest difficulties lie.

GENERAL CIRCULATION

If we look at our problem on the broadest possible scale—as it might be from another planet—we see a spherical shell of mixed gases held to the surface of Earth by gravity, showing complicated convective motions under the variable heating from the sun and, because it overlies a water surface, carrying a variable amount of water vapour that is continuously replenished by evaporation and deposited elsewhere as rain or snow. The motions are certainly complicated, but when local features are smoothed out and averages are taken, a very remarkable world organisation is revealed, and this we call the general circulation. Some of the picture was known in ancient times and the broad outline of prevailing surface winds was filled in when ships began to circumnavigate the world. In recent times the wide use of sounding balloons has given a good three-dimensional world picture through the whole troposphere and far into the stratosphere, and very recent work with rockets has taken the record to much greater heights, even to the fringe of the atmosphere .

The diagrams given in Figs. 1 and 2 are sufficiently self-explanatory to show the structure of the general circulation as it is known at present. The most remarkable features—charted only in the last decade and not yet fully explored—are perhaps the jet streams, the cores of strong wind which encircle the earth just below the tropopause. It has now become clear that at any one time there are numerous minor and limited jet streams associated with minor fronts but sub-tropical and polar-front jet streams are almost always present and showing in the averages. There are two distinct tropospheric maxima in each hemisphere. In addition there is a westerly jet stream high up in the winter stratosphere, but this is only indirectly linked with weather.

The exploration of the atmosphere everywhere with the discovery and charting of new and generally unexpected features has a fascination of its own for any true man of the weather, but the scientist must also ask deeper questions. Broadly speaking, the circulation at the present time is the same as it was a year ago; as much heat has

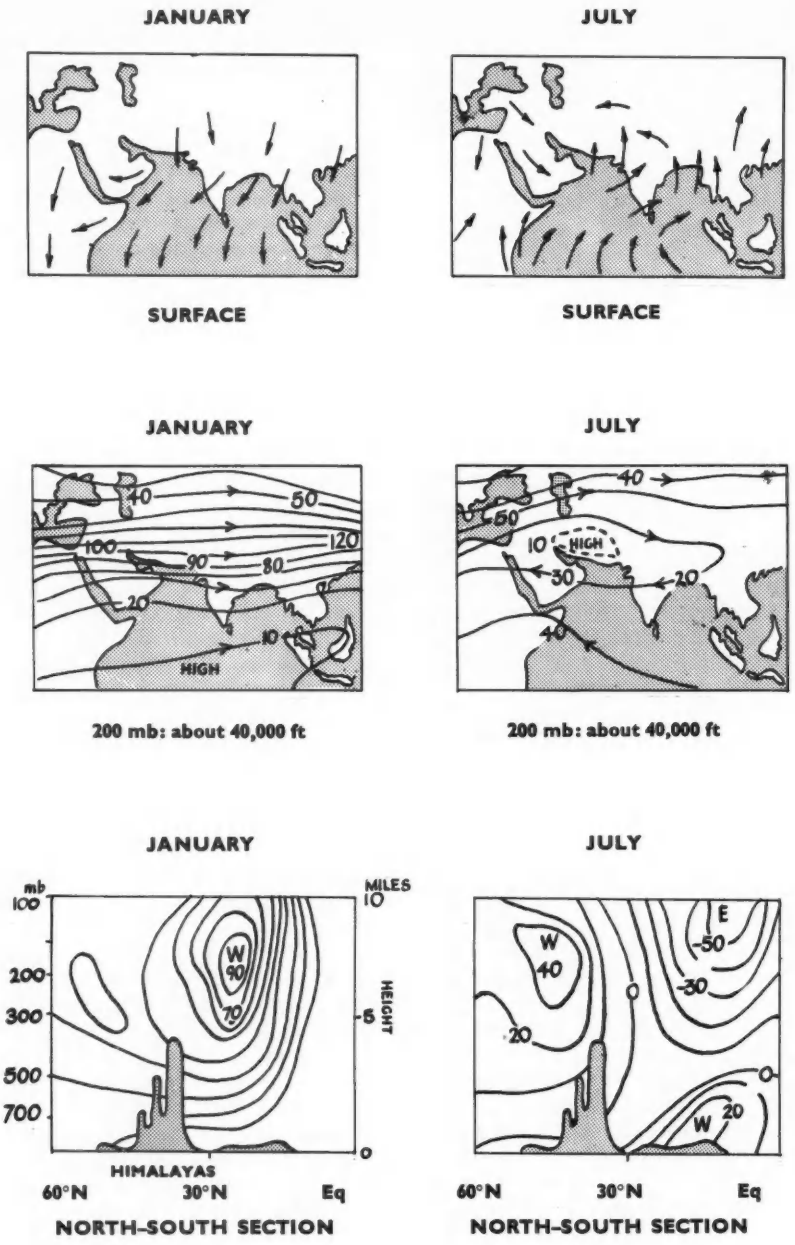


FIG. 3. MONSOONS IN THREE DIMENSIONS

The charts for January and July show the difference in wind régime from winter to summer. The surface charts show the familiar monsoon winds near the ground with a complete reversal over the Indian Ocean and its neighbourhood. At about 40,000 ft. above there is a change from a strong westerly, 100 knots near the jet stream, to an easterly wind over India at the time when the south-west monsoon is blowing at the surface. The north–south sections show the jet-stream core very clearly. In winter it is south of the Himalayas but in summer the westerlies are north of the mountains, with an easterly jet stream to the south. Note how the upper-level easterly jet stream is found high up above the low-level westerlies.

Numbers show mean wind speed in knots.

been gained as lost, as much water evaporated as precipitated, and as much angular momentum passed to the earth below by friction as has been taken from it elsewhere, for the atmosphere is neither warmer, nor wetter, nor faster-flowing than it was before. We have, in other words, a state of balance, a steady state, and a full understanding must allow us to say how the steady state is maintained, where and how the heat or moisture or momentum is put in, where and how it is lost and what the mechanism is which transports properties from one place to another to redress any local unbalance. A full scientific understanding will, of course, need to represent all these processes by mathematical equations and the solutions of the equations will then be a fundamental theory of the general circulation and a first step in the theory of climate.

It can be categorically stated that, while innumerable text-books give more or less plausible explanations of the general circulation, there is not one which contains a basic theory although, of course, this does not imply that the explanations are entirely worthless. Indeed, a great deal has been added to our knowledge of the mechanism since Jeffreys (now Sir Harold) and others proved some thirty-five years ago that the average circulation can be explained only if account is taken of the fluctuations as well. It is impossible to explain the distribution of energy, moisture, and momentum of the general circulation without taking account of the relatively smaller scale motions, just as it is impossible to explain the dispersal of smoke from a chimney without taking account of the small-scale turbulence in the air; for this reason, the depressions and anticyclones and other passing weather systems are often described as "synoptic turbulence" in the general circulation. If, then, anyone purports to show why we have doldrums, trades, and brave west winds and makes no reference to turbulent irregular motions, we may be sure that he is guilty of sleight of hand and needs to be watched very closely.

This problem of providing a basic theory of the general circulation is very properly regarded as the most important of meteorology and there is every reason to believe that it can be solved

in a matter of years if a sufficient number of able people can be wooed away from the blandishments of atomic nuclei and cosmic space. Important first steps have already been taken, especially by American workers, Phillips and Smagorinsky in particular, but the physics is complicated, the mathematics tough and the arithmetic taxing for even the most up-to-date electronic computers.

MONSOONS

The word "monsoon"—derived from the Arabic word for "season" and originally used for seasonal winds in the Arabian sea—has been found convenient as a general term for large-scale seasonal variations. It has even been applied to the winds in the high stratosphere which circulate from west to east in the winter hemisphere and east to west in the summer, but these are planetary circulations caused by the fact that the summer polar stratosphere in continuous sunshine is warm while the winter polar stratosphere in continuous darkness is very cold. Many meteorologists would like to reserve the word monsoon for the seasonal changes of a geographical character caused by the differences in thermal properties between continents and oceans.

The basic causes for all these seasonal changes are similar. For all ordinary meteorological purposes, the atmospheric pressure is hydrostatic and differences of temperature (or density) imply differences in the pressure. The first intuitive argument is that warm air, being less dense, will give a lower pressure at the earth's surface than cold air, but this is no more than half the story. By working hydrostatically from the bottom upwards, we could equally well say that warm air will give high pressure above and cold air low pressure above, since in hydrostatics density is linked not directly with pressure but only with the change of pressure with height. Misunderstanding on this simple matter has led to much fallacious argument in meteorology, although there would seem to be no excuse when it is so well known that the surface pressure in the warm air over the tropical oceans is generally higher than that in the cold air of the polar seas. The distribution of pressure in the atmosphere presents, of course, a dynamical problem for which the hydrostatic decrease of pressure with height is only one of the conditions to be satisfied; as a matter of fact, it is more generally true to associate high temperature with high pressure aloft than with low pressure below. It is as a consequence of this that upper winds in the troposphere (including the jet streams) are generally from the west, circulating cyclonically round the areas of low upper-air-pressure in the colder regions nearer the pole.

QUOTE

Earth satellites may show their greatest dividend by providing photographs from space of all tropical cyclones—and possibly even thunderstorms—as soon they begin to take shape.

Coming now to monsoon circulations, we need to explain how the winds are produced by the seasonal change in the differences in temperature between land and sea. Comparatively, the oceans change little in temperature through the year for the extra sunshine of summer is stored within a considerable layer of surface waters. Over the land, the summer heat is not stored but is given off very readily to the air with the result that, in relative terms, air over the land is warm in summer and cold in winter. The effect hydrostatically is for summer to have either low surface pressure or high upper air pressure, and when the dynamics of winds is introduced the solution is a compromise; both low surface pressure and high upper air pressure are found centred over the hot part of the continent. This arrangement permits the low surface air to converge and the upper air to diverge, giving a satisfactory dynamical model. It is extremely interesting to study the circulation of the Indian summer monsoon in this light and diagrams are given showing the flow at the surface and at 200 millibars (a height of about 40,000 ft. under average conditions) in winter and summer (Figs. 3 and 4). The strong easterly winds of summer at high levels over Aden are as genuinely monsoonal as are the south-westerly surface winds over India itself. The sections taken roughly north to south through Aden in April fully show the quite remarkable nature of the change, the easterly upper winds of July having a jet stream structure.

The monsoon changes over the world are a representation of the annual rhythm of weather and have far-reaching consequences for some regions, including north-west Europe, which may seem to be beyond their influence. Year differs from year and the onset of the Asiatic summer monsoon, whether dated from some criterion of Indian weather or dated from the onset of east

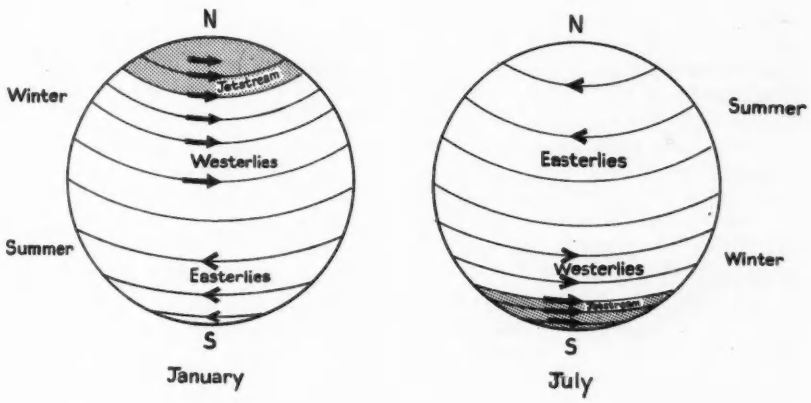


FIG. 4. MONSOONS IN THE STRATOSPHERE

At high levels, from 30–50 km. (20–35 miles) above the earth, there is a complete change from season to season with westerly winds in the winter hemisphere and easterly winds in the summer hemisphere. Near the edge of the polar winter darkness a stratospheric jet stream develops and disappears suddenly in the spring, with sudden warming of the stratosphere.

winds in the high atmosphere over Aden, is very variable, with a range of three weeks. Before the quantitative theory of the general circulation provided by mathematical physics is likely to help much in the problem of forecasting, it will surely need to incorporate in some way the monsoon changes. When we can understand and perhaps predict why one year differs so much from another, a great victory will have been gained.

DEPRESSIONS AND ANTICYCLONES

Weather maps such as those shown here and those seen in newspapers or on television invariably show, if they cover a large enough area, one or more regions of relatively low or relatively high pressure. Articles on the subject usually give the lion's share of attention to the depression, but the high pressure region is equally important and interesting, although as a region of subsided air that is relatively dry with little upper cloud or rainfall it is dynamically the simpler. It was recognised early that the depression was a region of rapidly changing winds bringing cool, dry air in its wake, but it was the Norwegians during World War I who codified the structure and brought forward in 1919 their famous cyclone model, since reproduced in every textbook. Briefly, their model was one of cold and warm air flowing side by side, the colder current naturally nearer the pole (polar air) and the warm current nearer the equator (tropical air), the two separated by a front. The cyclone development was correctly pictured as a large-scale eddy which swept up a portion of the warm air (warm sector) and replaced it by cold air, lifting the warm air from the surface in the process and so causing cloud and rain. On the whole, lifting means inflow or convergence to the centre which, due to the conservation of angular momentum, causes the air already possessing the spin of the earth to show greater spin as its area contracts. As a result, the air rotates relative to the earth in the same direction as the earth rotates round its axis, which defines cyclonic circulation. The model permitted systematic weather analysis and was a great benefit in practical forecasting, but it was not a completely formulated dynamical model and described rather than explained the phenomena.

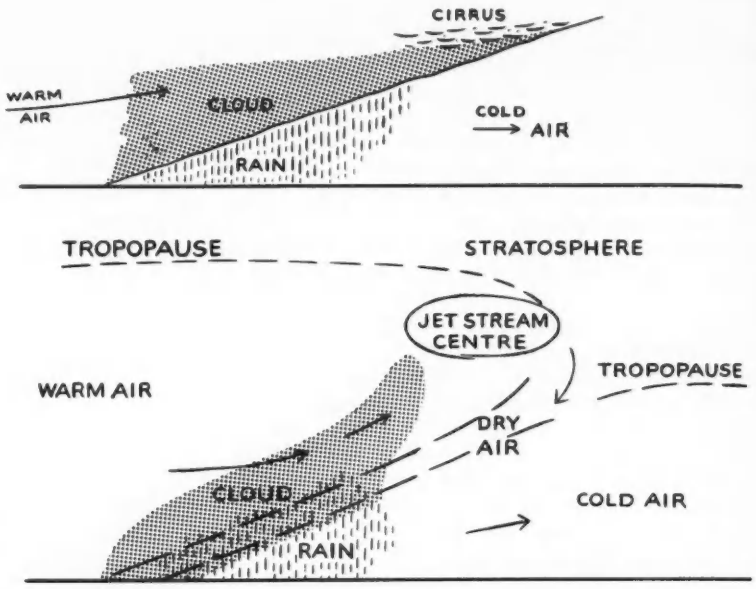


FIG. 5. WARM FRONTS—OLD AND NEW

The upper diagram is the conventional warm-front section, on the old idea of warm air sliding upwards over cold air. The lower diagram is a schematic section based on recent observations showing the break in the tropopause, the jet-stream centre and a complicated dynamical overturning with subsiding dry air in advance of the rising cloudy air.

The success of the years since World War II in providing a truly dynamical cyclone model is therefore a very important step forward. It has contributed much to modern forecasting and has been built into methods of dynamical prediction that have been developed with electronic high-speed computers. The dynamical model differs from the earlier front and air-mass model in that the sharp division between cold and warm air is replaced by a broad zone of transition—a baroclinic zone. In this way, the dynamics of continuous fluids may be used instead of that of surfaces of discontinuity, giving results which are more general if in some respects less precise and providing an anticyclone model as the complement to the cyclone. During this same recent period, techniques of three-dimensional analysis have been evolved and fully agreed upon internationally so that every large synoptic centre now has, in addition to surface synoptic charts, charts for many pressure levels—700, 500, 300, 200, 100 mb, and sometimes others.

The dynamical model includes a wavelike distortion of the surface flow and pressure pattern associated with a similar wavelike distortion of the temperature field lagging nearly a half-wavelength behind (to the west). Since, by the hydrostatic equation, the temperature field represents the difference between the pressure fields at two levels and so also the wind shear, the phase difference between pressure and temperature waves allows upper divergence to coincide with lower convergence and the whole system is a self-developing mechanism providing all the most essential features of the depression. Elsewhere in the pattern, where the wave distortions place convergence above a region of lower divergence, the air subsides, becomes dry and warm, and provides an anticyclone model.

Although the simple dynamical models have greatly helped our understanding of what is happening and have permitted prediction by calculation, the fronts which they omit are, nevertheless, phenomena of nature. These fronts have been subjected to much observation and analysis, revealing many interesting features relating to the tropopause, stratosphere, and jet streams as well as to remarkable tongues of dry air whose existence was hitherto little suspected. The structure

is indicated in Fig. 5. To build the detailed dynamics of fronts into the broader dynamics of the baroclinic atmosphere is an outstanding problem.

TROPICAL HURRICANES

The central and western parts of the tropical oceans are subject to a violent kind of depression or cyclonic storm, smaller in diameter but much more intense than our familiar depressions. Although they often bring rain where it is needed, these storms may be disastrous over a limited area. They have this in common with other depressions: a centre of low pressure and a wind circulation that is invariably in the same sense as the rotation of the earth (from west to east on the equatorial side); they are also associated with inflowing air near the surface, rising air above, and much cloud and rain. In many other respects, however, they are very different and have a different theoretical explanation.

Our depressions are produced because cold and warm air flowing side by side have a transition region that is unstable and liable to curl up into a whirl; the large baroclinic whirl may go in either direction, giving either a depression or an anticyclone. The tropical hurricane, by contrast, is caused by instability in the vertical and is driven by the latent heat liberated in rainfall; it has a warm core and apparently requires an exceptionally warm sea (with a temperature above 27°C or 81°F) to support it. These storms are scientifically remarkable; were the sea only a few degrees cooler, they might never occur at all, and if it were warmer they might, other things being equal, be much more frequent. In this respect, the atmosphere is delicately poised with potentialities for climatic change which have yet to be considered.

The effect of tropical hurricanes on the weather of other latitudes is not negligible, for the track of each storm is generally poleward near the continental coasts, bringing Japan and even Canada within their direct influence. Moreover, a number of our own depressions are triggered by hurricanes arriving from the south and the centre of low pressure of a cyclone near Iceland may sometimes be traced back continuously to a hurricane in the tropics a week or more earlier. To the theoretician, the problem is challenging and has been partially solved, but the curious structure with spiralling arms and a warmer inner core of dry subsided air (the eye of the storm) has not yet been deduced by calculation. For the countries frequently subject to their damaging violence—especially, but not exclusively, China and Japan, India, and the United States—accurate prediction is worth a great deal of effort, which it certainly is receiving in America. Perhaps it is in this context that the investment in meteorological earth satellites will show its greatest dividend by providing photographs from space of all tropical cyclones as soon as they begin to take shape.

QUOTE

A low pressure near Iceland may be traced back to a hurricane in the tropics a week or more earlier.

SHOWERS AND THUNDERSTORMS

While the tropical hurricane is a rare manifestation of vertical instability driven by the latent heat of condensation—so rare that the number present on Earth at one time is unlikely to reach ten and may fall to zero—the ordinary shower and its more intense variety, the thunderstorm, is commonplace, occurring in thousands at all times and contributing a large fraction, indeed, the greater part, to the rainfall of the world. Basic theory on the nature of vertical instability in a moist atmosphere has advanced little beyond the elementary stage reached during the last century although the structure of thunderstorms has been intensively studied by aircraft, radar, and radiosonde, revealing features previously only conjectured—in particular, the amount of water in clouds and the nature of the intense down-draught which, brought down by rain and cooled by evaporation, produces intense squalls at ground-level. It used to be stated that a large variation of wind with height would distort and destroy any large convection system, but since storms often occur in frontal regions where there is a large wind shear with height, it has been recognised recently that a dynamical model for a persistent storm may demand a shear. Dr F. H. Ludlam of Imperial College, who has devoted himself to the study of clouds, sees in the shear of wind a possibly essential feature of heavy hailstorms but, so far, calculated results based on a dynamic model have not appeared in the literature. The calculation of hail size, introducing the fall speed of the particles, as well as all the usual dynamical and thermodynamical factors, may be really formidable. Hail is traditionally the great destroyer in thunderstorms—far more important than the lightning except where forest fires are a hazard—but in recent years danger to overhead power lines and radio interference have enhanced the importance of the electrical manifestation. By means of radio direction-finding, lightning counters, and perhaps by observations from earth satellites, it is possible, in principle, to monitor all the world's thunderstorms continuously and in time this may be found worth the cost. The theory of thunderstorm electricity is still contested but a new theory put forward by Prof. B. J. Mason is very convincing.

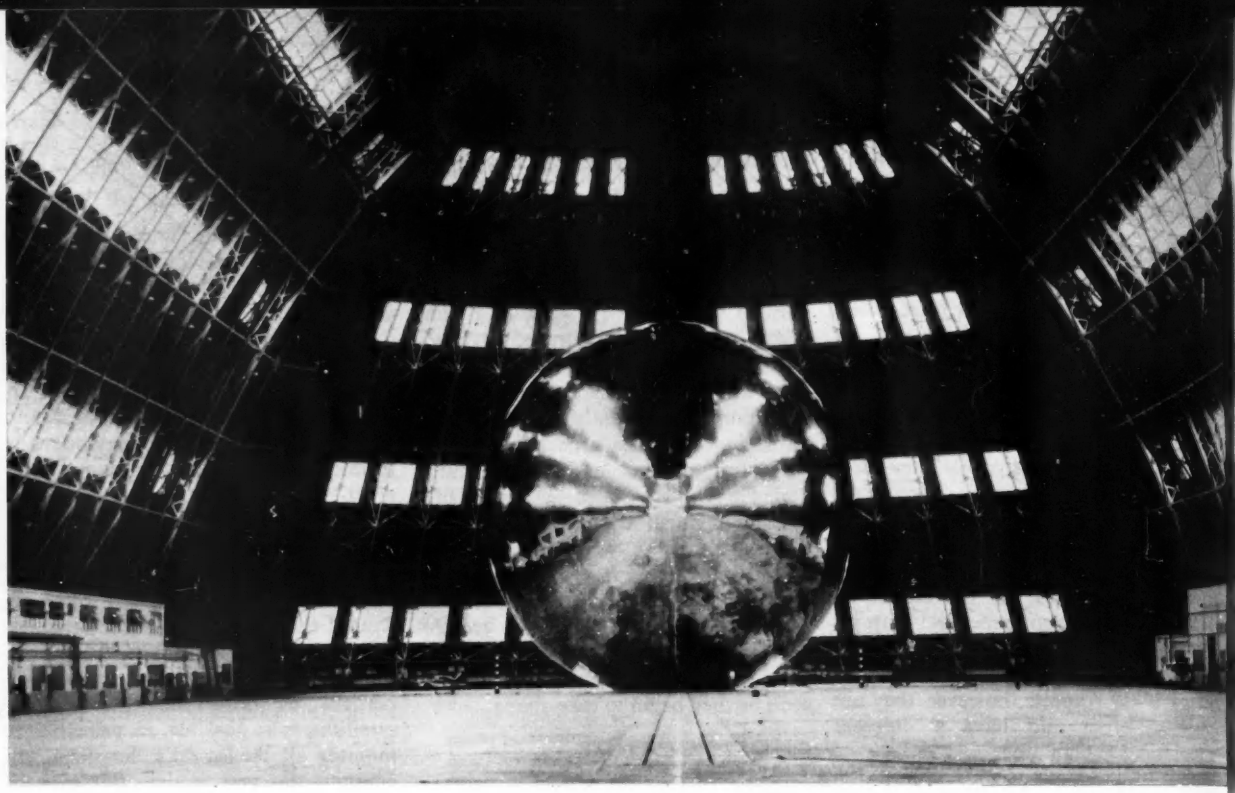
THE ATMOSPHERIC HEAT ENGINE

The dynamical systems which have been briefly described, from the general circulation itself down to small-scale showers, are, as it were, different units in the complicated inter-lock-up mechanism of the atmospheric heat engine.

In the process of degradation of energy from the input of short-wave solar radiation to the output of long-wave terrestrial heat to space, a small proportion is temporarily converted to the mechanical energy of the wind circulations which determine our weather and climate.

The mechanisms can be described with some certainty, and research must now complete the picture with satisfactory dynamical and, above all, thermodynamical theory. In the meantime, meteorology is pre-eminently a useful science which allows us to predict the weather within limits and even to dream of controlling it. Achievements and prospects in these practical pursuits are the material of another article.

In the next issue: Controlling and Forecasting the Weather.



COMMUNICATION SATELLITES

J. R. PIERCE

A system of many low, unoriented or simply oriented satellites is likely to be attempted before stationary satellites at 22,300 miles high are possible.

While science and technology have in many ways put all of the world within our reach, they have enabled us to talk across oceans only with difficulty, and to see across oceans not at all. Today there are 76 million telephones in North America and 58 million in the rest of the world. In highly developed land areas, and especially in North America, Europe and Japan, telephone operating companies or agencies provide teletype, data, facsimile and television transmission as well as telephone service by means of complex and highly integrated facilities. These include multipair cables, coaxial cables, and microwave radio relay systems. The coaxial cables and microwave systems are capable of transmitting thousands of telephone conversations and many television pictures simultaneously.

By contrast, the oceans have been spanned since 1927 by radio telephone links, and since 1956 by submarine telephone cables. Short waves cross oceans only by reflection from the ionosphere. Short-wave transmission is often of poor quality, and sometimes it fails completely when the ionosphere is disturbed by solar storms. Submarine telephone cables make use of conservatively designed, widely spaced undersea repeaters, which can operate on the ocean floor without repairs, and which can be supplied with power over the cable itself.

Although there are now cables to Scotland, France, and Hawii, the frequency bandwidth of *all* available trans-oceanic telephone circuits, radio as well as cable, is only about a hundredth that of circuits crossing a north-south line cutting across the United States in almost any location.

Dr Pierce is Director of Research, Communication Principles, at the Bell Telephone Laboratories, Murray Hill, New Jersey, U.S.A.

(*Page opposite.*) The hundred-foot Echo balloon, made of mylar, a half a thousandth of an inch thick coated with a reflecting aluminium film.

Trans-oceanic circuits are inadequate to the rapidly growing demands of telephony. Television signals take about 1000 times as much bandwidth as telephone signals. Short-wave radio cannot provide television transmission. Cables of the future might, but present-day cables cannot.

Microwaves travel in straight lines, right through the ionosphere, which bends short-wave radio waves round the earth. If a microwave radio repeater capable of receiving a signal and retransmitting it were placed in a satellite which was visible simultaneously from both sides of the ocean, we could use microwaves to provide the sort of broadband communication channels across the oceans that we now have over land. The complicated, flexible communication systems which already cover continents could be adequately linked together, and world-wide communication could be carried out as readily as continental communication now is.

THE FIRST PROPOSALS

The idea of satellite communication is not new. Arthur C. Clarke proposed satellites for relaying and broadcasting in an article in *The Wireless World* in 1945. He pointed out that a satellite in an orbit 22,300 miles above the surface of the earth revolves about it in one sidereal day. If such a satellite revolved in the plane of the equator, in the same direction as the rotation of the earth, it would hang stationary in the sky. Some one of three equally spaced satellites in such orbits would be visible from all points of earth farther than 8° from the poles. Clarke proposed placing manned space stations in such orbits and equipping them with both high-power broadcasting and low-power relaying equipment.

When I first wrote about satellite communication in 1955 I discussed reflecting spheres, such as *Echo I*, and oriented reflecting mirrors, as well as satellites equipped with radio receivers and transmitters. I considered satellites either in 22,300-mile-high stationary orbits, or in lower orbits in which they would revolve more rapidly, rising and setting. In this case, one would need to launch tens of satellites in order to assure that at least one would be mutually visible at two ground terminal points almost all of the time. The ground-terminal antennae would have to track such satellites as they moved across the sky.

In a paper delivered in London in 1960, at a meeting of the International Scientific Radio Union, Walter Morrow of the Lincoln Laboratory proposed an even simpler sort of satellite, or satellites—a belt consisting of multitudes of fine wires a half wavelength long, circling the earth like a ring of Saturn. In this case the ground-terminal antennae would point at a common portion of the belt, and many dipoles would reflect a signal from transmitter to receiver. Different orbiting wires would, of course, serve as reflectors at different times. The position of the belt of wires could be stationary but not the positions of the wires forming it.

These constitute the general sorts of communication satellites which have been proposed. It is interesting to note that both Clarke and I overlooked one important matter in connexion with 22,300-mile-high stationary satellites: the time it takes radio waves to travel to and from the satellite.

TECHNICAL DIFFICULTIES TO OVERCOME

The propagation time to and from a 22,300-mile-high repeater is about a quarter of a second. Thus, a speaker cannot receive a reply to a remark for about half a second. Tests indicate that this delay may not interfere with a telephone conversation if a four-wire circuit is used—that is, if completely

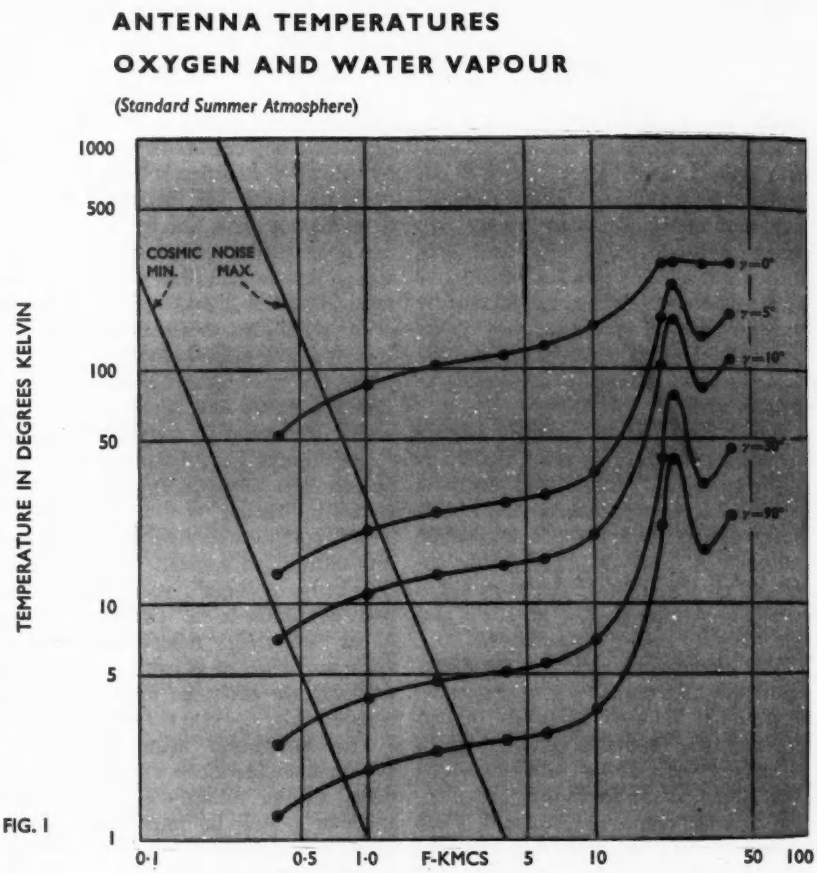


FIG. 1

Temperature due to oxygen absorption seen by an ideal antenna as function of elevation angle ø and frequency.

independent paths are provided from each transmitter to each receiver.

Actual telephone circuits, however, always include two-wire links, over which one both talks and listens. When a round-trip delay of half a second is included in such a circuit, an intolerable echo precludes use. To get rid of the echo, one must use an echo suppressor which interrupts transmission at one end while the party at the other end is talking.

Echo suppressors do not seriously interfere with a conversation when the round-trip delay is less than a tenth of a second. Some very preliminary experiments indicate that circuits with half a second delay and echo suppressors may be objectionable to a considerable fraction of users, and that a conversation is sometimes brought to a halt by both users speaking at once. Only further investigation will show how serious this really is.

The realisation of satellite communication depends on the exploitation of two arts. One of these is a new, expensive, and uncertain art of space. We can judge this art in part by the facts that a satellite launching costs several million dollars, and that only half of our launchings have been successful. Satellite communication can come into being only through this art, only through the use of vehicles which have already been developed at great cost by the government primarily for other purposes. Even alterations of existing vehicles are technically very risky as well as costly.

The other art is the powerful, mature art of electronics. This has provided us with powerful transmitters, sensitive receivers, huge antennae, complex computers and tracking systems, and subtle methods of reception. This art has advanced greatly since Clarke wrote about satellite communication in 1945, and considerably since I wrote in 1955.

The electronic space payload draws upon the new and old arts. Repeated operational failures show us that it presents new problems. The American Army's Project *Score* satellite which was launched on December 18, 1958, and *Courier*, launched October 4, 1960, contained not only radio receivers and transmitters, but tape recorders for receiving and repeating messages. *Score* was an early experiment of which no great life could be expected. *Courier* operated for only 18 days. This ridiculously short life seems terribly discouraging, but it is illuminating as well. It shows that there are real problems to be solved in operating electronic equipment in space.

SUCCESS WITH GROUND INSTALLATIONS

The speed and success with which the *Echo* communication experiments were carried out is an excellent instance of our ability to deal adequately with electronic and mechanical problems *on the ground*.

Despite the fact that *Echo*'s orbit changes rather rapidly, it has been possible to track the satellite to within ± 0·2° automatically by means of pointing directions computed from orbital observations.

Only about a hundredth of a millionth of a billionth of the 10,000 watts beamed at the *Echo* satellite is reflected into the receiving antenna. None the less, by use of a maser receiver which adds only as much noise as is equivalent to a temperature of 24° centigrade or Celsius degrees above absolute zero, by using a horn reflector antenna which does not pick up thermal radiation from the hot earth, and by using broad-band frequency modulation and a special narrow-band FM with feed-back receiver which tracks the modulation of the signal, it was possible to provide a high-quality telephone circuit. Telephoto and speedmail signals were also sent via *Echo*, and signals were sent across the ocean to Jodrell Bank, the Royal Radar Establishment, and the British Post Office in England, and to CNET in France.

The complicated *Echo* ground communication equipment was brought into being in about a year. Its features can be valuable in connexion with either passively reflecting satellites such as *Echo*, or active satellites. Its performance could be improved upon considerably.

The knowledge acquired in connexion with the *Echo* experiment and many of the techniques and devices used in it will be important in any satellite communication system.

Among the knowledge, we can count the calculation and experimental checking (by D. C. Hogg) of the sky noise due to thermal radiation from atmospheric oxygen and water vapour. Noise temperatures for various angles of elevation are plotted *versus* frequency as the curved lines in Fig. 1. We see that sky noise is low between 1 kMc and 10 kMc/s, provided that we never point our antenna lower than about 7° above the horizon. It is undesirable to point the antenna close to the horizon anyway, as this would increase interference between satellite ground microwave terminals and ground microwave systems. Frequencies above 10 kMc/s are increasingly undesirable for satellite communication because of increasing noise. Further, thermal radiation from rain rises very rapidly with frequency, even below 10 kMc/s.

Power is extremely expensive in space, partly because weight is proportional to power, and partly because high-power equipment tends to have a shorter life than low-power equipment. Thus, the techniques and devices used to conserve power in the *Echo* experiment will be important in connexion with active satellites as well. Among these are the maser amplifier, low-noise antennae such as the horn-reflector antenna, broad-band modulation such as FM, and receivers which can operate at a low signal-to-noise ratio, like the FM with feed-back receiver used in the *Echo* experiment.

Further, the tracking by means of predicted satellite position which was used in the *Echo* experiment will be important in any satellite system which does not make use of 22,300-mile-high stationary satellites.

These things we have, but much remains to be done before we will have a useful, operating system using active satellites. The dependability, the life of satellite electronic equipment is of primary importance to satellite communication. The chief initial cost of a satellite communication system will be the cost of launching satellites. Unless the life of satellites is many years, the chief operating cost will be the cost of replacing satellites. The practicability of satellite communication depends on obtaining long life in the novel and hazardous environment of space.

Most active space payloads depend on solar cells, together with storage batteries, for electrical power. Unfor-

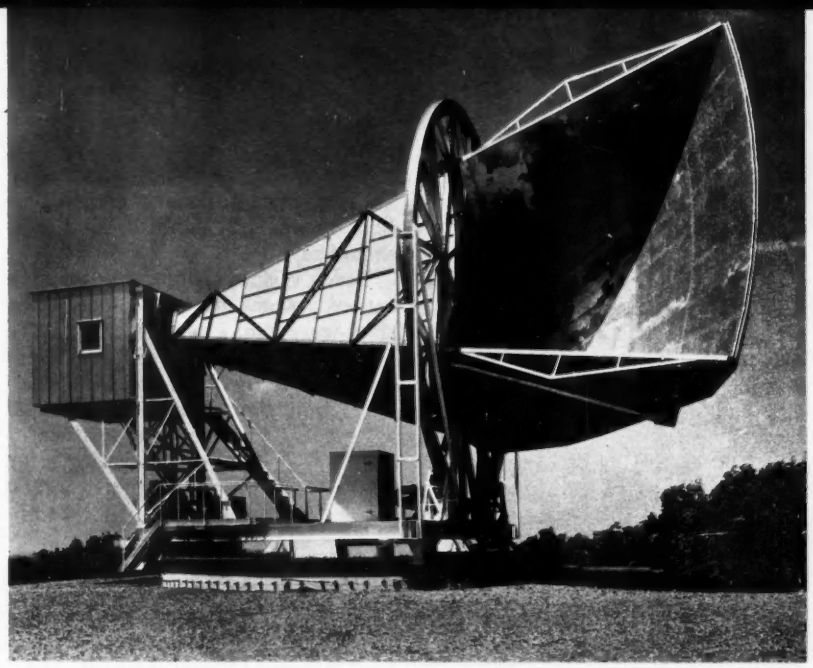
tunately, radiation pervades space. Cosmic radiation has little effect on solar cells. Less than a tenth of an inch of glass, quartz, or sapphire will shield solar cells adequately from the electrons of the outer Van Allen belt, which is most intense about 13,000 miles above the earth's surface. It appears impossible to protect solar cells against the effect of the protons of the inner Van Allen belt, which is most intense at an altitude of around 2000 miles and which extends to an altitude of perhaps 5000 miles.

While the discovery and exploration of the Van Allen belts is a major scientific feat, the extent, intensity, and composition of the radiation have not been carefully measured, nor their changes with time accurately determined. This makes our best present estimate of life uncertain by a factor of perhaps 12. The solar cells in a satellite a few thousand miles high might last several months or several years. Solar cells on higher satellites would last somewhat longer.

Satellite electronic equipment faces other hazards, including problems of acceleration, cooling, and temperature cycles. Undoubtedly, long life can be attained in space. The *Vanguard* transmitter is still functioning after three years in orbit. This is a very simple, very low-power transistor oscillator, and enough solar cells were used to keep it going despite a drastic fall in their efficiency. In general, we can say that it is possible, though not easy, to make *simple low-power* electronic equipment operate reliably for many years without readjustment or repair. This is not true of complicated equipment or of high-power equipment, such as television broadcast transmitters.

One could make a trans-oceanic communication system by using satellites a few thousand miles high with nearly isotropic antennae. The satellites could be either unoriented or spinning. If one is to use satellites farther from the earth, it is desirable that they be equipped with a directional antenna which will beam power just to the disc of the earth rather than broadcasting it into space in all directions. This requires keeping the antenna pointed at the earth.

Changes in pointing could be made by spinning small flywheels in the satellite, thus causing the bulk of the satellite to turn in the opposite direction. Or gas jets could be used to orient the satellite.



The horn reflector antenna used in the Echo experiment. This antenna does not pick up thermal radiation from the ground behind it.

At altitudes of a few thousand miles, the earth's magnetic field is strong enough to serve as a reference or to provide an appreciable force in orienting a satellite; this means of orientation has not been adequately explored experimentally. Or we might make use of another force in orienting a low satellite. For a given angular velocity, centrifugal force increases with increasing radius of orbit, while the force due to gravity decreases with increasing radius. Imagine a satellite made up of two equal weights tied together with a wire and aligned radially with respect to the earth. The centrifugal force will be greater on the outer weight, and the force of gravity will be greater on the inner weight. There will be a small tension in the wire. If displaced, the system will tend to return to its radial orientation. But the force is very small and the problem of damping out oscillatory motions is great.

If a 22,300-mile-high satellite is to hang stationary in the sky, the initial orbit must be corrected, and subsequent readjustments in orbit will be necessary. These can be carried out by means of gas jets or other impulse-producing means under radio control from the ground.

At this time, satellites have been oriented for relatively short periods only. We have nothing but the wildest conjectures as to what orientation systems will work at what altitudes and whether they can be made to work for periods of years.

THE FIRST TYPE OF OPERATING SYSTEM

In view of these problems and uncertainties, what will the first operating satellite system be like, how will it come into being, and when will we have it?

I feel reasonably certain that an active satellite transmitter suitable for around 600 telephone circuits or a television signal will use a low-power transmitter of around 2 W. This low power can be used only with a broad bandwidth, say, around 40 mc., and low-noise receivers (with masers) fed by low-noise antennae on the ground.

Ultimately, 22,300-mile-high stationary satellites will certainly be tried, but technically they seem somewhat remote, and the transmission delay of the long path may mitigate against their use for telephony. I believe that we will have a system of many low unoriented or simply oriented satellites without station-keeping first.

The Bell Telephone Laboratories, the Bell System's research and development organisation, is working towards

such a system. The work includes the fabrication and testing of solar cells, travelling-wave tubes, transistors, and other components, the construction of an experimental satellite, work on orientation, and other matters, and the construction of a ground terminal near Rumford, Maine. The American Telephone and Telegraph Company has announced that it is willing to bear research costs, including launching cost, and its share of the cost of an international satellite communication system. It is hoped that an experimental satellite can be launched in 1962.

Various other companies, including General Electric, the International Telephone and Telegraph Company, RCA, Hughes, General Telephone and Electronics, and Lockheed have proposed various satellite communication systems.

NASA's Project Relay contemplates the launching of an experimental active communication satellite capable of television transmission in 1962.

The Air Force plans an experimental launching of a belt of wires, as proposed by Morrow. If there are no unforeseen difficulties, this could give us a very simple communication system. Reflection from many different wires will cause some fading. A belt of wires would not provide as good and broadband a transmission path as single satellites would.

The goal of the Army's Advent programme is a system using 22,300-mile-high stationary satellites, with orientation and station-keeping.

Thus, many satellite communication proposals have been made and several government contracts have been let. Yet the concrete, experimental progress which has been made is limited. There has been too much grand planning and too little experimental work. Many technical problems must be solved if some of the proposed satellite communication systems are to become practical and economical, and even the simplest systems take us to the very edge of our demonstrated ability.

METHOD OF OPERATION AND PAYMENT

Technical problems are not the only problems that must be solved if we are to have useful satellite communication. Satellite communication will be most useful in linking together continental communication systems which are already highly developed. How could an international satellite communication system be operated, and who should pay for it?

Today various continental communications systems are linked together by short-wave radio and by cables. The operating companies and agencies of each country own and operate the terminal facilities in that country. Cables are owned jointly by the agencies or companies which provide service in the countries which the cables connect. Problems concerning frequency allocation are worked out through the International Telecommunications Union, which is now a part of the United Nations.

The cost of an international satellite communication system would be large, but no larger than the cost of a number of underseas cables. Telephone cables now connect the United States with Great Britain, Continental Europe, Hawaii, Alaska, Puerto Rico, and Cuba. Cables are being built from Puerto Rico to Antigua and from Florida to Jamaica. A third and broader band transatlantic cable, to Great Britain, is planned for 1963, and a cable between Hawaii and Japan for 1964. The British Commonwealth plans a world-wide cable network. The cost of satellite communication could well be borne in the same manner as the cost of such cables.

It would seem natural that international satellite communication should be brought into being and operated just as our present trans-oceanic telephony by short-wave radio and cable was. After all, space science and technology are merely a part of a larger world of science and technology.

New satellite ground terminal under construction near Rumford, Maine.

NEW PROGRESS WITH BOUNDARY LAYER CONTROL

ROY ALLEN

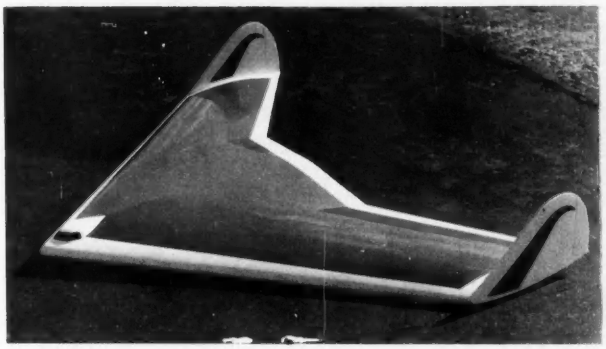


FIG. 1. The HP 117 model for long-range passenger operations at low cost. The fuselage of the conventional transport has been discarded in favour of an all-wing machine which can be entirely laminarised. If built, the HP 117 would weigh less than a Boeing 707, yet carry twice as many passengers over the same distance.

One of the most promising ideas for extending the range of transport aeroplanes has been proved in theory, but has hitherto received little official encouragement. Now both British and American work is being given support, and the tide is turning.

Anyone who is vaguely familiar with the science of aeronautics will know that when a cambered plate is pushed through the air it will rise due to the difference in pressures on the top and bottom surfaces. Revealing films taken over the years showing the behaviour of different-shaped aerofoil sections have given designers great insight into lift and how to employ it.

Much else has been learned about air flow over surfaces, of course, and one thing that has been for long apparent is that even the cleanest of aerofoils produces a turbulent wake. The degree of turbulence in the airflow will depend on many things, such as the shape of the aerofoil, its speed through the air and surface smoothness, but despite the appreciation of this by designers and a considerable expenditure of effort on the part of research establishments to produce "laminar flow" wings, the problem of turbulence remains with us on the current generation of aeroplanes.

Turbulence involves drag, influencing an aeroplane's performance—slowing it down, reducing its effective range and limiting its payload. The object, then, is to eliminate as much of it as possible.

A more efficiently functioning wing could offer the promise of ocean-spanning performance by an aeroplane carrying several hundred people. At least two aircraft companies on different sides of the Atlantic have appreciated this, and Handley Page Ltd, of England, have done the most work towards achieving this goal.

Because of the smallness of their budget, however, and because governmental financial backing has not been readily forthcoming in the past, the British company has been able to complete only a test programme, while the American company, Northrop Corporation, is now reaching the point of producing an aeroplane with United States Air Force backing.

The two companies' approach to improving performance has been simple. Stemming from the discovery by the German scientist Ludwig Prandtl that suction could be used to control the "boundary layer" of air around a wing, the companies set about making wings that would incorporate a method of sucking off the boundary layer air and so eliminate turbulence.

Four years ago, Handley Page issued details of an aeroplane which they proposed for long-range civil or military transport work and which, their studies had shown, could fly non-stop from Britain to Australia if boundary layer control were employed.

By today's standards the aeroplane was conventional in appearance, having

Mr Allen is on the staff of Aeronautics.

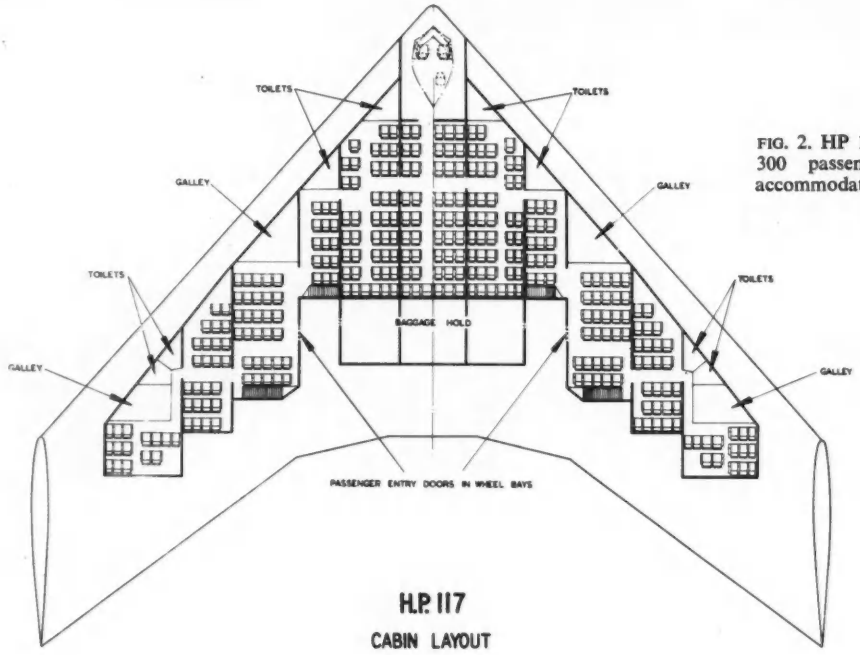


FIG. 2. HP 117 cabin layout. 300 passengers could be accommodated.

an orthodox transport fuselage and swept-back flying surfaces. It would be powered by four Rolls-Royce Conway by-pass engines mounted in packs beneath the wings. The wing span was 198 ft. and the fuselage length 135 ft., a rather greater span than the jet transport of today but about the same size cabin. This last might be expected, for the designers, requiring a basis for planning, had taken the Boeing 707 fuselage design and fitted their laminarised surfaces around that.

All of the surfaces of this aeroplane would be laminarised—the tail surfaces as well as the wings. This in itself illustrates how important the company regarded the absolute elimination of boundary layer drag. By means of a built-in suction system, the turbulent air encountered just above the surface of the planes would be drawn away, leaving the airflow laminar, that is, passing in smooth layers around.

The performance gains estimated for this aeroplane were remarkable: the stage distance possible with a 30,000 lb. payload (about 120 passengers) against a 30-knot headwind was 9000 nautical miles, or about twice that of a conventional jet transport. Alternatively a greater payload might be carried for a reduction in range. And the direct operating costs were estimated at 1·5*d*. per seat stat. mile.

This Handley Page project did not get built, however, and the company does not grieve today, because studies have shown that much more can be attained if the principles of boundary layer control are used in a different manner. The aeroplane proposed by the company today, known as the type HP 117, is of radical conception (see Figs. 1 and 2). It is an all-wing design, of 125-ft. wing span and capable of crossing the Atlantic with 300 passengers. As shown by the diagram, these passengers would be carried in the fore and central parts of the wing, while fuel, services, controls and the engines would be removed and separate from the passenger areas. The Ministry of Aviation has awarded a contract to Handley Page for the construction of a flying scale model of the HP 117.

THE NEW DESIGN

The company's first step in boundary layer control was the construction of a "glove" wing, which followed the contours of a normal aeroplane wing but had small-diameter holes in its top surface through which turbulent layer air was sucked. This glove wing was fitted to a de Havilland Vampire aircraft and flight tests made. Parallel tests were made at that time in the United States by Northrop, under the direction of Dr Werner Pfenninger, with an F-94 Starfire aircraft. The results were conclusive enough to show that both companies were on the right track.

The next step by Handley Page was to design the long-range transport aeroplane mentioned, with a conventional fuselage which could not be laminarised. This had the drawback that while the flying surfaces of the aeroplane could be fitted for suction, the fuselage offered substantial profile drag. Thus the concept of the all-wing aeroplane was re-born.

A feature of the new design is its aerodynamic cleanliness. The top surface of the wing is free from "blisters" save the pilots' canopy, and the suction slit for boundary layer control does not appear until after this point. For boundary layer control, surfaces must be smooth—and indeed have a high finish. Another point of interest on the model is its vertical fins at the wing tips for achieving directional control.

But the most significant step made in the development of a successful boundary layer control system has been the change-over from perforations in the wing's surface to narrow-gauge

slots. These are 0·005 of an inch wide and run span-wise across the aeroplane's wing, their actual number depending on the size of the wing and the extent of control of the boundary layer air necessary.

Surface smoothness has been mentioned as one of the more important factors influencing performance of the laminar flow wing. In the early days of the glove wing and up to the time of the long-range transport, surface excrescences were extremely troublesome, one of the main problems being how to rid the leading edge of wings of common flies.

A variety of answers were presented, including the fitting of paper strips to the wings, which covered the perforations of the BLC system. When the aeroplane reached a desired altitude, the angle of the wing was changed to let air under the paper's edge, whereupon it was torn off by the airstream, and the BLC system allowed to function.

This was a palliative, however, and the final solution to keeping the surface free of insects came by accident, as is often the way. One of Handley Page's Victor bombers was being used for BLC tests one afternoon when the pilot reported back to Dr G. V. Lachmann, director of research, that the day's work had been a failure. The explanation he gave was that the Victor had flown through a rain cloud and the flies impacted on the wings had been completely washed off.

Dr Lachmann's own response to this was immediate: "Marvellous! Then we must find a method inducing water into the airflow at the wing leading edge in order to wash it free of insects as required." The solution that has been proved and since adopted as standard is to inject water into the airstream at a pressure of 80 lb./sq. in. forward of the leading edge, where it is blown back on to the wing's surface with sufficient force to free the wing of all insects.

The all-wing type of construction has a great deal to offer apart from the fact that its surface can be entirely laminarised. For one thing, it means the fuselage with its non-contributing structure weight can be eliminated. The HP 117 project has no greater wing span than the Boeing 707, while its take-off weight is less. The former means that hangarage problems are simplified and the latter that the aeroplane could operate from conventional-length runways at world airports. Its payload would also be higher.

In the case of supersonic all-wing transports, which are a logical development of this sub-sonic machine, the low take-off weight will have an important bearing on the "sonic boom" created. The sonic boom is a function of aircraft weight. The lower an aircraft's weight the less shock-wave it will create as it passes through the sound barrier. All of these things add up to an impressive future for the laminarised aeroplane.

Some problems remain to be solved. There is at the moment, for example, a protracted study being made of the best method of power to be employed for drawing off the boundary layer air. Should the power plant be an auxiliary one carried in the aeroplane? Or should it be an integral propulsion-cum-bleed air power unit?

In the meantime, Northrop Corporation are building a test aeroplane modified from a Douglas B-66, and this is expected to fly in September 1962. If the potentialities of this advanced system were fully realised by the Ministry of Aviation by then, perhaps another Anglo–U.S. race could be sparked off.

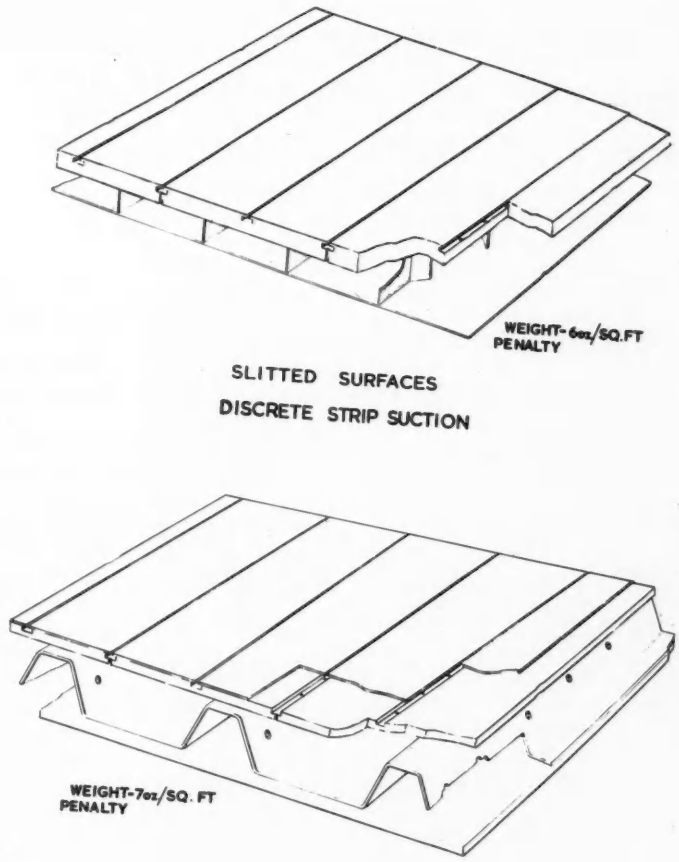


FIG. 3. Handley Page wing constructions for a laminarised aeroplane. The transverse slits for boundary layer suction are shown on the top surface of the structures, with their ducts for taking the bled air into the interior of the wing. Two methods for supporting the top skin are shown, the girder type construction producing a slightly higher weight penalty.

HUMAN AROUSAL AND EFFICIENCY IN PERFORMING VIGILANCE TASKS

D. E. BROADBENT

Recent studies have thrown new light on the relative importance of: the probability of receiving signals; a change in background stimulation; the state of sleeplessness; the time of day; and the length of watch.

There are words in everyday use which recognise that a man may be "excited" by a rapidly changing situation, or that a monotonous job is "unstimulating" and so may send him to sleep. In recent years a physiological mechanism which may be the basis of such changes has been discovered and studied intensively. As has long been known, stimuli reaching the sense-organs are transmitted upwards to the cortex by pathways which are specific: light falling on a particular part of the retina produces activity at a particular point in the occipital region of the brain, light on another region of the retina has an effect at a different point, and so on. But in addition it has become clear that stimulation may produce changes that are widespread in the nervous system, and which are mediated through other, non-specific, pathways passing through the reticular formation at the base of the brain.

Damage to these pathways can cause drowsiness, and it seems likely that the reticular formation is closely involved in the normal arousal and de-arousal of the brain which occurs in waking up and going to sleep. One can plausibly suppose, therefore, that a person who is being stimulated a good deal, who is standing in a well-lit room listening to a variety of minor sounds, will have all his higher nervous processes facilitated by a plentiful barrage of impulses from his reticular formation: while another person who is reclining relaxed in a dark room in complete silence, and who is therefore relatively unstimulated, will lack this facilitatory background and so will be generally inefficient.

It is, however, one thing to show that stimulation of the senses produces a generalised increase in responsiveness, as measured by electrical activity, in the cells of the brain, and quite another to show that this factor is important in ordinary human life. One could argue that stimulation will indeed keep a man awake, but not alter his behaviour once he is awake: one could argue that localisation of function in the central nervous system is ill understood, and that therefore behaviour may depend upon parts of the brain which have not been studied: or one could admit a minor effect on behaviour of sensory stimulation and yet argue that other factors are more important in deciding human efficiency. What evidence, therefore, is there from experiments on behaviour, to see whether efficiency goes up and down with some general level of arousal?

Such evidence is most likely to be found when men work on their own, at a job which does not call for much muscular effort with its consequent self-stimulation, and in surroundings which involve little variety of external stimulation. These conditions may sometimes occur in the monitoring of instrument panels, in industrial inspection, or in the keeping of a radar watch.

Such kinds of work, under the name of "vigilance tasks", have been the subject of interest for many years because performance in them does appear to be unduly inefficient. Sometimes the inefficiency is constant from the beginning of the work-period to the end; but it is quite often found that the performance gets worse as the time spent in the situation increases. Can the inefficiency found in vigilance tasks be put down to a lowered state of arousal?

Dr Broadbent is Director of the Applied Psychology Unit of the Medical Research Council, Cambridge.

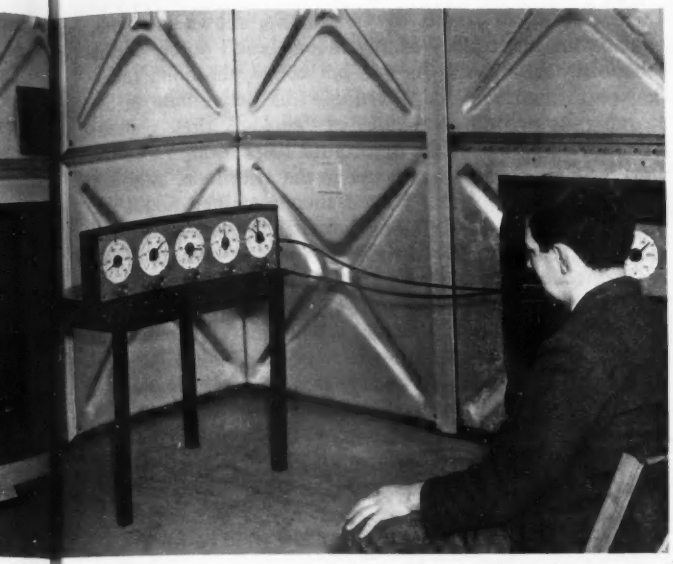


FIG. 1. A subject watching a large array of dials for occasional "dangerous" readings. The experiment is in fact one on noise: hence the loudspeakers visible behind the dials.

THE IMPORTANCE OF PROBABILITY

One obvious implication of an arousal theory is that a man will be better able to detect signals on a radar screen, or abnormal readings on a dial, if such signals occur more often. This is indeed true, as has been shown in a number of experiments but particularly by Jenkins of MIT. It is often therefore suggested that human efficiency should be kept high in practical situations by increasing the signal rate, perhaps by putting in dummy signals if no real ones are occurring. But although the basic fact is undoubted, it can also be interpreted in ways that have nothing to do with arousal.

For example, an increase in the signal rate makes more probable the occurrence of a signal at any instant: and it is a general principle that the more probable of two stimuli will be the easier to identify. A spoken word can be heard through a loud noise if the listener knows that only two or three alternative words are possible: while a much lower noise-level is necessary if one is to hear a word drawn from a large vocabulary. Thus in vigilance tasks the effect of signal rate might be directly due to the accompanying changes in signal probability.

The two factors can, however, be separated by a technique used by Colquhoun of Cambridge. Using a task resembling industrial inspection, as shown in Fig. 2, he treated his subjects in three different ways. Some people were given many objects to inspect in a given time, and half of the objects were faulty. Other people were given the same large number to inspect, but only a few were faulty. A third group of people had the same small number of faults to find as the second group did, but only had a small total number to inspect so that half of those inspected were in fact faulty.

If men are aroused by the appearance of signals ("faults"), the first group should do better than either of the others: or if arousal is raised by having many objects to inspect, the last group should do worse than either of the others. In fact, the second group was the worst and the other two are roughly equal. That is, efficiency was high when signal probability was high even for a small total number of signals.

It is hard to explain this result in terms of arousal, although it could perhaps be done by some *ad hoc* hypothesis postulating that non-faulty objects reduce arousal while faulty ones raise it. Even such a postulate will not explain Colquhoun's finding that the differences between the groups appeared as soon as the experiment was started, before differences in the stimulating properties of the situation would have had time to work. (His subjects had previously been practised, using the same incidence of faults as in the actual experiment and thus began with an appropriate knowledge of the signal probability—it has since been shown by Colquhoun and Baddeley that, if practice takes place with a high signal rate, performance in the actual experiment will start off very similar whether the rate then used is high or low. With a low rate in the actual test, it will, however, deteriorate more rapidly with time, as the man becomes familiar with the low signal probability.)

It does seem, therefore, that the probability of a signal is a real factor distinct from the arousing effects of signals. Much of the inefficiency in vigilance tasks might well be

FIG. 2. A task used by Dr Colquhoun to study the efficiency of inspection. Small circles appear six at a time in a slot; sometimes one of the circles is slightly abnormal in colour, and if so the subject has to report it by pressing a button. The rate of presentation of circles and the number of abnormal ones can be varied, so as to find the conditions most conducive to efficiency.

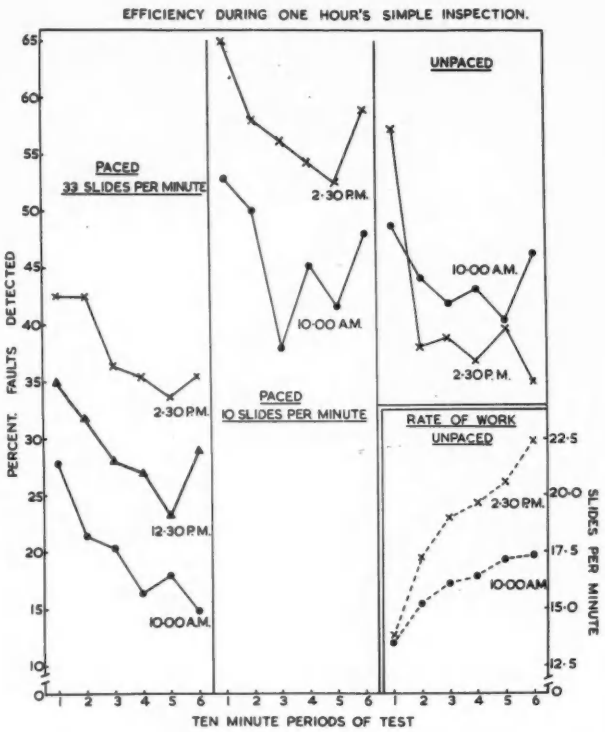


FIG. 3. Some of the results obtained with the task shown in Fig. 2. A "slide" is a set of six circles. Note the effect of time of day, which shows that some factor of arousal is important. Also note the equal amount of deterioration, within a test, found at widely different initial levels of efficiency: this argues against the view that such deterioration is due to the subject changing his estimate of the probability of signals occurring.

due to the fact that they are situations in which signals are improbable: and, as noted parenthetically above, it is even clear that an inappropriately high initial expectancy on the part of the man may cause his performance to deteriorate during the run as has sometimes been found in vigilance tasks. Nevertheless, there are some results that cannot be explained by probability.

NEED FOR A CONCEPT OF AROUSAL

One such result has been provided by McGrath of Los Angeles, who compared performance in detecting visual signals with and without a varied background of medium-level sounds. He also studied the detection of auditory signals with and without the provision of interesting visual pictures to be studied at the subject's whim. In both cases detection was better when the level of irrelevant stimulation was higher.

This result cannot be due to anything about the job itself, so that something more than the probability of the signal is involved. The same conclusion is forced on us by the results of Wilkinson in Cambridge showing that performance of vigilance tasks (unlike many other kinds of work) is worse after the loss of a night's sleep.

Another set of results by Colquhoun, shown in Fig. 3, indicate the inadequacy of any theory based purely on the probability of signals. As can be seen, Colquhoun found that performance in the afternoon was better than in the morning, at two different rates of "pacing", that is mechanical presentation of the objects for inspection. When the work was unpaced, it was faster in the afternoon, although the proportion of faults missed then remained about the same as, or even a little more than, in the morning. (These results naturally apply to the case in which the same job has not been done in the morning and afternoon, so that the afternoon is not contaminated by fatigue effects from the morning. Also Colquhoun found that differences in personality interact with the effects of time of day.)

It is, of course, well known that many physiological functions have a diurnal rhythm reaching a peak in the afternoon, and falling to a trough at the notorious hour of two a.m. or thereabouts. Not only do Colquhoun's results show that a factor outside the task itself influences performance: they also illustrate that in this task there is a consistent downward trend during the period of work.

If this trend were due to the abandonment of an inappropriate estimate of signal probability, the size of the trend would surely be different for different initial levels of efficiency. The afternoon workers, seeing many of the signals, ought not to revise their estimate of signal probability as radically as the morning ones. The latter see few signals and so ought to deteriorate more in performance. However, deterioration is similar for all groups.

It seems clear, therefore, that the downward trend cannot be due to revised estimates of signal probability. In any event, Colquhoun's subjects show deterioration during a run even though they have been previously trained with the appropriate signal frequency.

Thus the average level of efficiency can be shifted up or down by changes in the background stimulation, and by changes in the state of sleeplessness or time of day: which are variables associated with the physiological mechanism of arousal. Furthermore, the deterioration during a watch-period, which is sometimes found, is not always due to a factor of signal probability. Can an arousal theory explain this deterioration from start to finish of a watch?

One can expect that as any situation remains the same for a long period, the sense organs will adapt and the various stimuli present will have less and less arousing effect. Indeed, this has been shown by physiological means: and thus one would expect arousal to be lower at the end of watch than at the beginning. Those vigilance tasks which show no deterioration might then be regarded as unusually arousing, perhaps because of their difficulty or the number of places which have to be scanned for signals. If the average level of efficiency on such tasks is low, that could be ascribed to the low probability of signals, since the latter factor has to be allowed some part. But despite the plausibility of this formulation, there are difficulties.

HIGHLY STIMULATING SITUATIONS

Broadly speaking, the snag is that the deterioration during a watch may appear as markedly under highly arousing conditions as it does in less stimulating ones. In

one of Colquhoun's experiments, seven signals occurred every ten seconds, so that the task might be regarded as one of frenzied activity rather than somnolence. But a deterioration still occurred during the run. Poulton of Cambridge showed deterioration with particularly easy or with particularly difficult types of signal, but not with intermediate ones.

A particularly clear instance is the work of Sanders and Ferrari of Soesterberg in Holland, who studied the performance of men who were looking for changes in the brightness of a flashing light. In simple conditions there was no deterioration during the run: but if a whole panel of lights were flashing and the signal involved a change in the brightness of several of them, then although initial performance was the same, a deterioration occurred during the run. Yet the latter condition should surely have been the more stimulating and arousing.

OVER-AROUSAL AND COMPETING RESPONSES

A similar point appears when we consider background stimulation, rather than that from the task itself. While McGrath's moderately loud (75 db) sounds improved visual vigilance, very loud (100 db) noises have been shown, both by the present writer at Cambridge and by Jerison at Wright Field, to impair performance. The impairment is greater at the end of the watch: thus the deterioration within the run is greater in these circumstances. If noise at such high intensities is arousing, one would expect the deterioration to be less.

One way out of the difficulty might be to deny that noise is arousing, and to argue that it is on the contrary a monotonous and so a de-arousing environment: it blots out the ordinary variety of minor background sounds. There are, however, several reasons for rejecting this view: physiological signs of high arousal have been reported as present in noise, some of the effects of noise appear to be opposed to those of loss of sleep, and Jerison has shown that the effect of noise appears only on a vigilance task with a high signal rate from several dials and not on a similar task with a low signal rate from one. We may therefore accept that noise is arousing, and are left with the problem of the inferior performance which it produces.

It is a normal feature of theories relating arousal to efficiency, that they should regard work as becoming better with increasing arousal only up to a certain point. Beyond that stage most theories postulate that arousal becomes too high, and efficiency positively declines; the man is in a flap. The disadvantage of supposing a relationship of this form is obvious: it makes the theory very difficult to test, because any change in conditions may perfectly well produce either greater efficiency or less.

Unfortunately, however, there do seem to be adequate grounds for including this slippery concept in any theory: Wilkinson, for example, has shown that men doing a certain task of mental arithmetic will, when incentives are applied, show a rise in their muscle tension and a drop in their ability to do the sums. In the same situation, men who have lost sleep will show a rise in tension but a rise also in their performance. This kind of result can hardly be explained except in terms of a limiting level of arousal, beyond which efficiency deteriorates.

Once such an idea has to be accepted, it becomes available for explaining poor performance in noise conditions—or, indeed, in any other highly stimulating environments. One would still expect, however, that as the work-period continued, habituation to the stimuli would cause arousal to drop, and therefore performance to improve. The fact that noise affects primarily the end of the work-period is not explained simply by supposing that over-arousal takes place.

It is doubtful what happens when arousal gets too high. One possibility is that many incorrect responses occur and interfere with the one which is correct at any given moment. If, for instance, we condition a bird so that it responds to light of a certain colour, we can increase the number of correct responses it makes by increasing its hunger; when it has been deprived of food for some time, it will respond more reliably to the light. If light of other colours is presented, there is a slight tendency for a response to appear to these colours too. The incidence of such responses also increases as the bird gets more hungry. The relative number of incorrect responses, however, becomes first less and then more as hunger increases. There is an optimal level of hunger at which the ratio of correct to incorrect responses is greatest.

Another example of the relationship between motives and errors is to be found in studies of differences between

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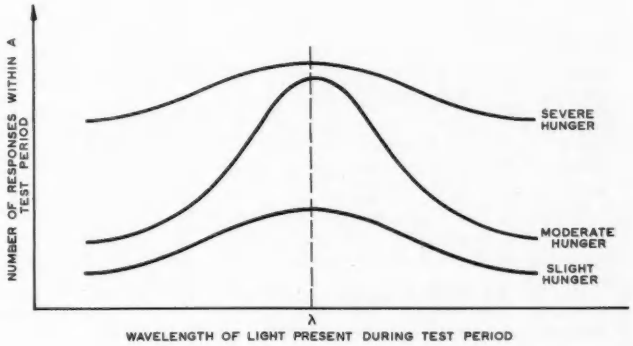

FIG. 4. If pigeons are taught to peck for a food reward in a light of wavelength λ, their rate of responding in lights of other colours is rather less. As the time since the birds last ate increases, the rate of response also increases; but the relative rate in "incorrect" colours of light first decreases and then increases again. At very severe levels of deprivation there is a marked tendency to react to lights other than the "correct" one. Thus very arousing situations may produce a rise in errors as well as in responsiveness. This result has been shown by various American workers.

THE BOOKSHELF

The Birth of the Bomb

By Ronald Clark (*Phoenix House, 209 pp., 16s.*)

Many people know that the first atomic bombs were made in the U.S.A. But it is not so widely known that a good deal of the research and development that was necessary to establish the feasibility of atomic weapons was done in Britain, and this book was written in the first place to tell the story of that British contribution. So it refers not so much to the birth but to the early pre-natal life of the bomb.

It is one of the frightening things in our day how fast a new piece of scientific knowledge can be developed into something that can profoundly influence our lives. Even Lord Rutherford, right until his death in 1937, did not believe we should ever learn how to use nuclear energies on a large scale; yet there is a curious story, according to Clark, that he warned Lord Hankey that nuclear physics might one day be of importance for the defence of the country, though he could not say in what manner. Lord Hankey was the chairman of the government's Scientific Advisory Committee which in September 1941 (less than three years after the discovery of fission) reported to the government that, after questioning many scientists who had done experimental and theoretical studies of the question, they concluded that an atomic bomb could and should be made.

The scientists who had done this work were probably the queerest collection of people that ever worked together on a war project. Only a small fraction of them were British; by the time in mid-1940, when the problem began to be taken seriously, practically all British physicists were already engaged in war research, chiefly on radar. So a number of refugee scientists (some of whom had been instrumental in pointing out the wartime possibilities of uranium fission) were gradually and hesitantly brought into this work, which was co-ordinated by the specially created MAUD committee (the origin of that name is in itself an entertaining story).

Some of the most dramatic episodes in the book have to do with heavy water; how the entire stock of that precious fluid was secured from Norway (the only country then making it); how it was brought over to England while France was being overrun by the German armies; how, in order to deprive Germany of heavy water, the Rjukan factory was sabotaged and eventually destroyed by the allies. Dramatic also was the arrival of Niels Bohr, who had fled from Denmark to Sweden in a small boat and had then been flown to England in the bomb bay of a Mosquito bomber.

All this and much more is described by Ronald W. Clark, in a vivid and readable style. He has clearly done a great deal of painstaking research, and I have found no obvious inaccuracies and very few misprints. Questions of physics are treated rather lightly; to a layman, some of the significant distinctions, say between slow and fast neutrons, may well remain unclear. An atmosphere of tact and neutrality prevails; when two of the people he questioned gave different accounts he preferred to quote them both rather than stir up muddy waters. Perhaps it is too early for that; perhaps too many feelings would be hurt by an account that dug down to the roots of the great controversies, to the clash of personalities, to the mistakes made and opportunities missed. In the meantime we shall be very glad to have this book which paints on a broad canvas, one of the most exciting and fateful episodes of our time.

O. R. FRISCH

Carcinogenesis

By I. Heiger (*Academic Press, 138 pp., 35s.*)

This book might well have had the title of "Some Theories Concerning Cancer and Carcinogenesis". With the vast and ever expanding literature on cancer-inducing agents one can sympathise with the author's statement in the preface that "No writer in such a wide field can be expected to be at home with, let alone confidently assess, the results of all the specialised lines of research, whether experimental or theoretical, which have become absorbed into the study of carcinogenesis."

It cannot be said that the author has contributed to this end, for indeed the major part of his book is devoted to a dismissal of all the theories without suggesting anything to replace them. It is no doubt salutary to demolish all ideas and thus stimulate new approaches but it is doubtful if the present author has the necessary background to do this successfully. Researches of great merit are dismissed summarily and phrases like "the logic of this statement is beyond the comprehension of the author" abound. It would seem, though in this field it is to some extent excusable, that the comprehension of the author is often limited by lack of awareness of current trends and insufficient reading of the relevant literature.

One cannot discuss all such omissions but the conception of specific firm protein-binding as a primary stage in chemical carcinogenesis may be taken as an example. The author's iconoclastic approach makes him certain that there is no firm evidence favouring this hypothesis. This is just not correct. Important facts such that the speed of attainment of maximal binding and its persistence, is correlated with the carcinogenicity of the azo dyes and the lack of the liver tumours and tissues other than liver to bind or metabolise dye, are not mentioned.

The author seems unaware of all the work suggesting that cell surfaces and specific protein synthesis on the RNA template in this area may be linked with the carcinogenic process. He does, however, deal fairly adequately with theories involving the role of cytoplasmic particles (including plasmagenes, proviruses, and viruses) in carcinogenesis.

On chemical carcinogens, where one would expect some attempt at an adequate account, the details are slender and confusing. Much space is devoted to the difficult problem of quantitative assessment of carcinogenic potency. This is important, for the validity of some theories, including that of a specific electronic configuration in the carcinogen molecule, depend upon the accuracy of the potency assessments. The extensive experimental details bearing on this point do not, however, fit into the framework of the book which is largely concerned with theories.

Other aspects dealt with are the multistage hypotheses and the famous work by Warburg on anaerobic glycolosis as the mainspring of the cancer cell. My immunological theory of cancer is presented but not very accurately, my involved style being blamed for this. It is given a modified blessing!

Great stress is laid on the importance of spontaneous malignant transformation of untreated cells in tissue culture. This view is shared by the reviewer, who has often pointed out, though this is not men-

tioned, that if, as it seems, chemical carcinogens do not cause malignancy in tissue culture, this points to a role of the intact organism in the chemical induction of cancer.

It has to be remembered that any such book by a single writer would probably receive criticism at different points by specialists in the field. It has the merit that much of it will be understood by the scientific layman and, if he accepts the conclusions that no current theories are adequate, make him think anew on this key biological problem. Moreover, it is far from dull as the various theorists, one by one, are trounced by quoting their own tautological statements. It is very true that the ultimate test of any theory of chemical carcinogenesis "is its capacity to predict the structural requirements for a super-carcinogen". H. N. GREEN

Director of Cancer Research, Leeds and Sheffield

Introduction to Nuclear Science

By Alvin Glassner (*Van Nostrand, 213 pp., 28s. 6d.*)

This text is based on lectures and demonstration experiments given to high school science teachers in the U.S.A. by members of the Argonne National Laboratory during the summers of 1957–8–9. Seven chapters are on nuclear physics, four are on engineering aspects of nuclear energy, and the final one is on biology and medicine. This is followed by eighteen experiments that can be made with simple inexpensive equipment.

Edison

By Matthew Josephson (*Eyre & Spottiswoode, 511 pp., 42s.*)

This new biography of a man who was impatient with theory, and yet was one of the most prolific inventors of all time, shows how Edison was a key figure in the evolution of modern technology. Always concerned with the commercial uses of his discoveries, Edison was thrown into close and sometimes angry contact with the financial barons of nineteenth century America, an age of unchecked and pitiless competition in which success justified all means. Jay Gould, J. P. Morgan and others of their kind were equally ready to back or break an invention according to where the quickest profit lay.

This study of the relation between technology and capitalism in its first, fiercest, chemically-pure state chronicles some of the exciting events of American industrial development, such as the bizarre "telegraph war" between Jay Gould and Western Union and Edison's bitter fight with George Westinghouse's company over alternating current.

The Young Scientist's Companion

By Maurice Goldsmith (*Souvenir Press, 160 pp., 15s.*)

Written for the youngster who is just starting to study science and who feels he would like to become a scientist eventually, this book gives an indication of the nature of scientific activity and lists books worth reading, places to visit, careers in science, and organisations which will supply him with materials free or at low cost.

LETTERS TO THE EDITOR

Sir:

Though a mere layman, I read with much interest your article on the rival merits of the "Explosion *v.* Steady State" theories (May, page 205) though I confess that I find the former incomprehensible.

I have seen no attempt to explain how all matter could have got into such a state, or how long it remained like that before the "Explosion".

If this occurred it must have had a locality with regard to the pieces ejected. If this was anywhere within a sphere of 10,000 million light years with the earth as centre, then it took place between us and some distant galaxy X. In that case galaxy X would be receding from us far faster than galaxy Y on the other side of us, even if at the same distance, but apparently this is not the case.

If the explosion occurred outside our observable sphere 10,000 million years ago, then the galaxies on the further side of our sphere have travelled—always more slowly owing to the effect of gravity—a distance of over 20,000 million light years in 10,000 million light years, or twice as fast as light, which we can surely dismiss as impossible.

Again no one seems to have discovered any small relic or nucleus of this former great concentration, and there always remains some part of a nova, and even of a super-nova.

The continuous creation theory seems to me to run into no such impossibilities.

E. G. S. CHURCHILL

Northwick Park, Blockley, Gloucestershire

Vortex Down Under

Sir:

Dear, delightful James! I have not spent such an enjoyable five minutes for a long while, and you, Sir, have never published a more important contribution (p. 178, April). Now we know where we are.

H. G. P. TAYLOR

Hempstead, Essex

Sir:

Having just read the article entitled "The Evolution of Humans on other Planets" by Professor William Howells, I should like to congratulate the said author on an extremely sensible and interesting composition. Speaking as a regular reader of DISCOVERY and more or less a layman from the point of view of Biology and Evolution, it is hoped that there will be more of such articles in the future.

T. G. R. FARTHING

47 Grove Road, Fishponds, Bristol

Sir:

The May issue of DISCOVERY contains a review of our publication "Management Training: Aims And Methods".

Would you please note that this book was written by *R. J. Hacon, B.Sc. (Econ.)* and not Patrick Meredith as shown in your review. C. W. DAVIES

The English Universities Press, 102 Newgate Street, E.C.1

B.Sc.
Degree

Study at Home

The increasing importance of Science in industry, commerce and education opens up many worth-while opportunities for Science graduates. Wolsey Hall (est. 1894) provides postal tuition for the examinations leading to the B.Sc. degree of London University, for which residence at the University is not required. **Prospectus** (please mention examination or subject) from E. W. Shaw Fletcher, C.B.E., LL.B., Director of Studies, Dept. WM8,

WOLSEY HALL OXFORD

HUMAN AROUSAL AND EFFICIENCY

Continued from page 317

different men, carried out by Spence and his associates in Iowa. People who show high manifest anxiety are reported by them to do well at jobs which involve only the learning of one response, with no great risk of error. An example is simple conditioning. They do badly, however, at jobs which involve a marked risk of making a wrong response. Such a job might be, for instance, learning to reply to each of a set of code-words by giving a different appropriate code-word, several of the responses being closely similar so that errors are easy to make. At this kind of task very anxious people do less well.

Since, then, there seems to be a tendency for errors to increase in highly motivated states, it is plausible that the bad effects of too much arousal are due to such errors. The aroused man might on this view be described as responsive but distractible. If so, then it is not unreasonable for the effects of over-arousal to appear only after work has proceeded for some time.

In animal experiments again, it is usual to find that the elicitation of one response reduces the probability of that response for a time, and hence increases the probability of other competing responses. Such a process would apply to the correct response in states of high arousal, and so the incorrect responses would have a better chance of breaking through after the situation had persisted for some time.

On such a view, highly aroused men would start out working efficiently and then begin to make errors towards the end of the work-period. This kind of approach is speculative, although it serves the purpose of showing that a modified theory of arousal might yet be consistent with the poor performance found towards the end of work in highly stimulating situations. It is clear that an unmodified theory will not do.

SUMMARY

To summarise, then, bad performance in unstimulating situations is not all due to low arousal. A job which is monotonous is often one in which crucial stimuli are improbable and this, rather than any general state of lowered efficiency of the nervous system, is the reason for poor response to these crucial stimuli when they do occur. Furthermore, the work of a man watching for occasional inconspicuous signals is often even worse in very stimulating circumstances than it is when the surroundings are quiet. This "distracting" effect may be due to over-arousal: it can hardly be due to under-arousal.

Nevertheless, there remains a core of instances in which men do fail to carry out their tasks because they have been without sleep, are at a trough in their diurnal rhythm of activity, or are in particularly unstimulating circumstances. Arousal is not the only and all-important variable in human efficiency: but it is a real one.

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the necessary calculations and automatically adjust the parameters in the simulation programme. As with other applications, everything depends on the speed with which the calculations must be done, the frequency with which adjustments are necessary, and the complexity of the calculations.

This constitutes a form of feed-back control, in that plant results are used in calculating optimum conditions. But it is not feed-back control in the strict sense. If the results obtained at the calculated optimum settings are not in accordance with expectation, the computer is powerless. It could, of course, carry out a further analysis of the records and recompute optimum conditions. But if the discrepancy is due to some cause of which no account is taken in the simulation—a mechanical fault, an instrumental error—no improvement will be achieved. In such a case only some optimum-seeking procedure such as evolutionary operation can achieve the currently best results.

CONCLUSION

The above is necessarily a very brief outline of some of the conditions for control of plant operations. Methods of control fall into three main categories:

(*a*) Simulation, leading to prediction of optimum conditions.
(*b*) Automatic optimum-seeking, that is, evolutionary operation, in which everything is done by the computer.
(*c*) Analysis of process records, whether normal or experimental, with a view to improving the accuracy of prediction.

These are fields in which the use of computers may be justified, and in which investigation into the economics of using them should proceed. A few "on-line" computers are stated to be in operation, all, so far as is known, being essentially simulators using feed-forward control. It is not possible to say in general whether such applications will pay off; it is necessary to study individual plants, which vary enormously in complexity and in the amount of information available on which to construct a simulation.

It will often be found valuable to investigate the process to the stage of making a worth-while simulation; in fact it is probably necessary to do this before an assessment of computer-assisted control can be made. Different processes may justify an on-line computer, an off-line computer, or no computer at all. Much will depend on their scale and complexity and on the amount of information available. It seems likely that most large-scale operations at least will benefit from some form of computer assistance and from the closer study which they will usually require before the matter can be adequately decided.

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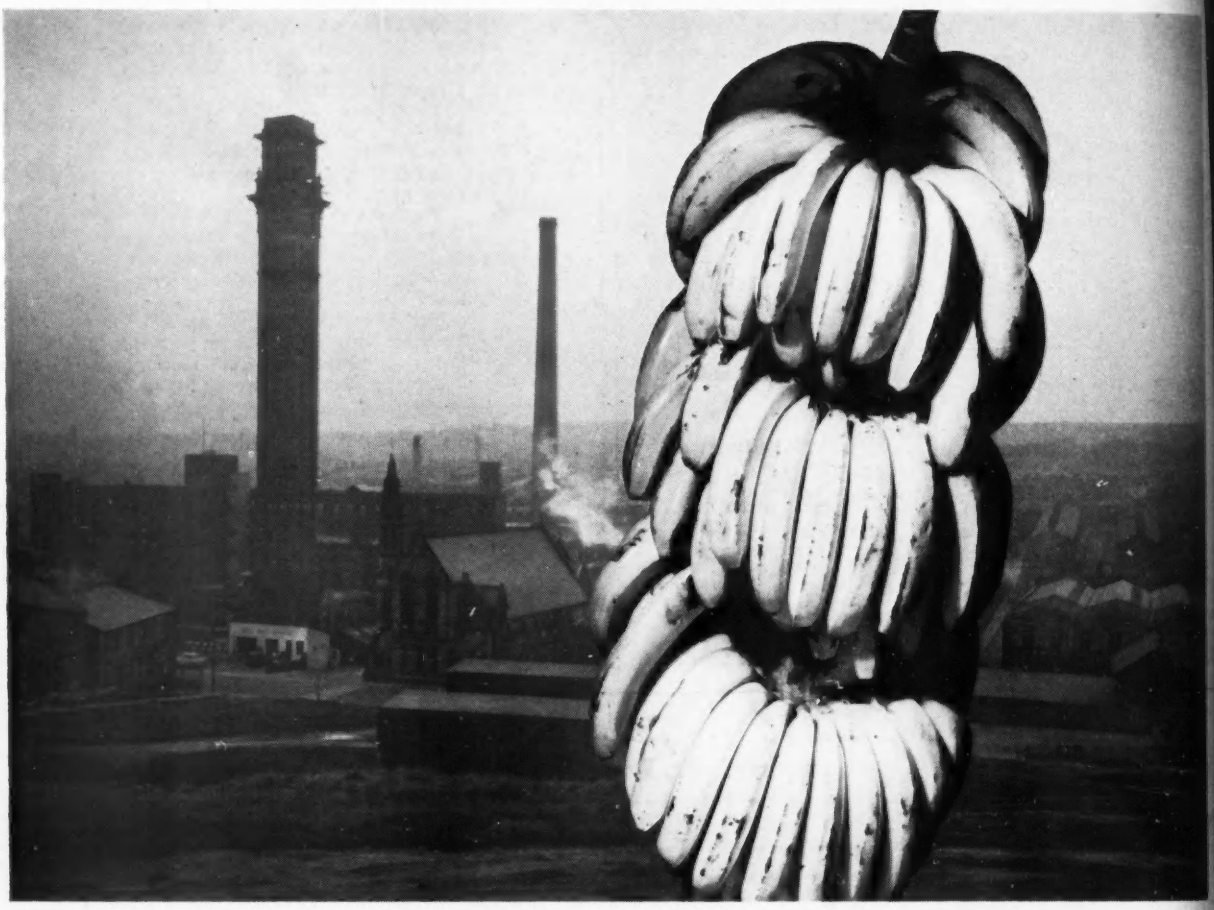

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